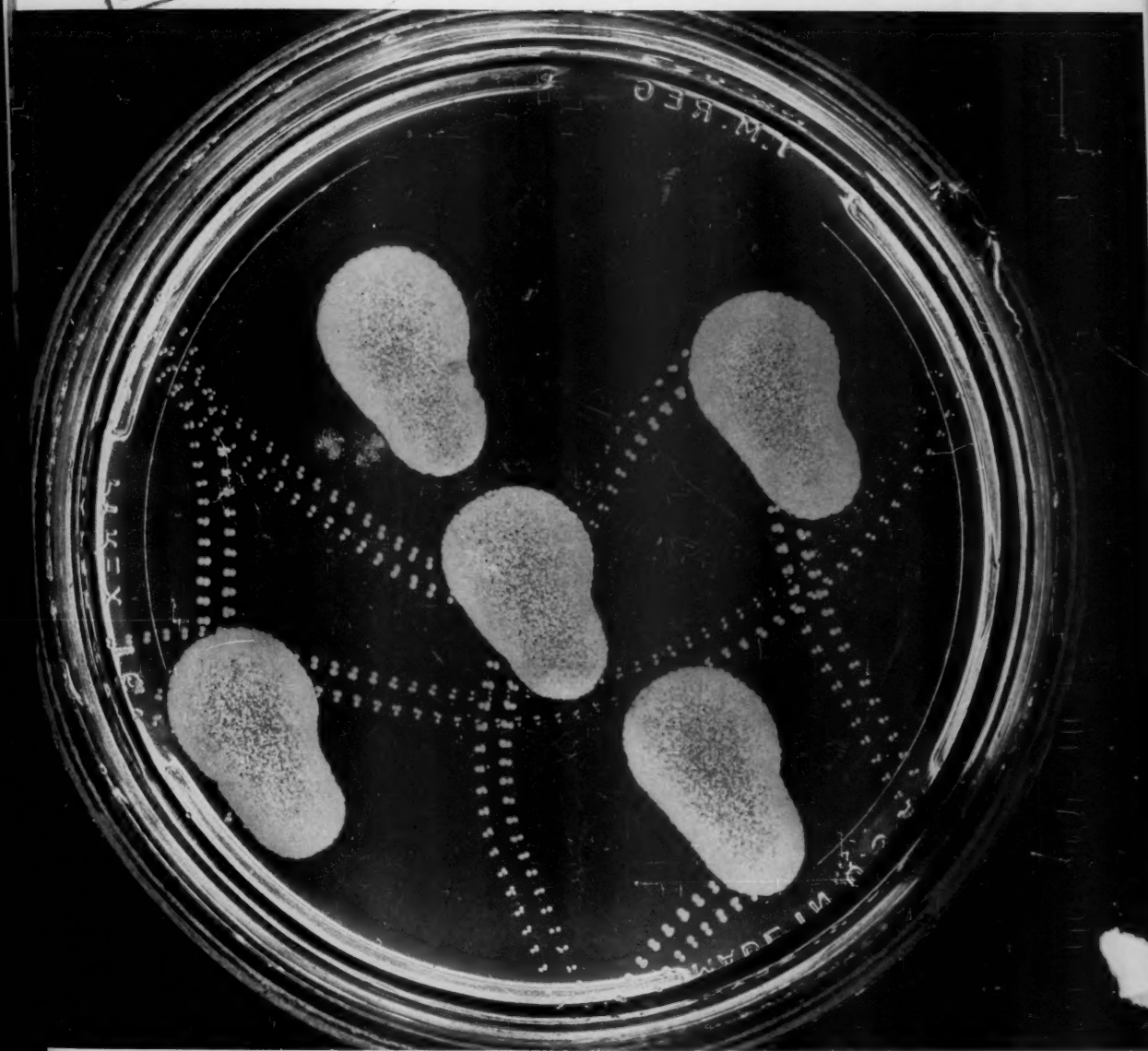
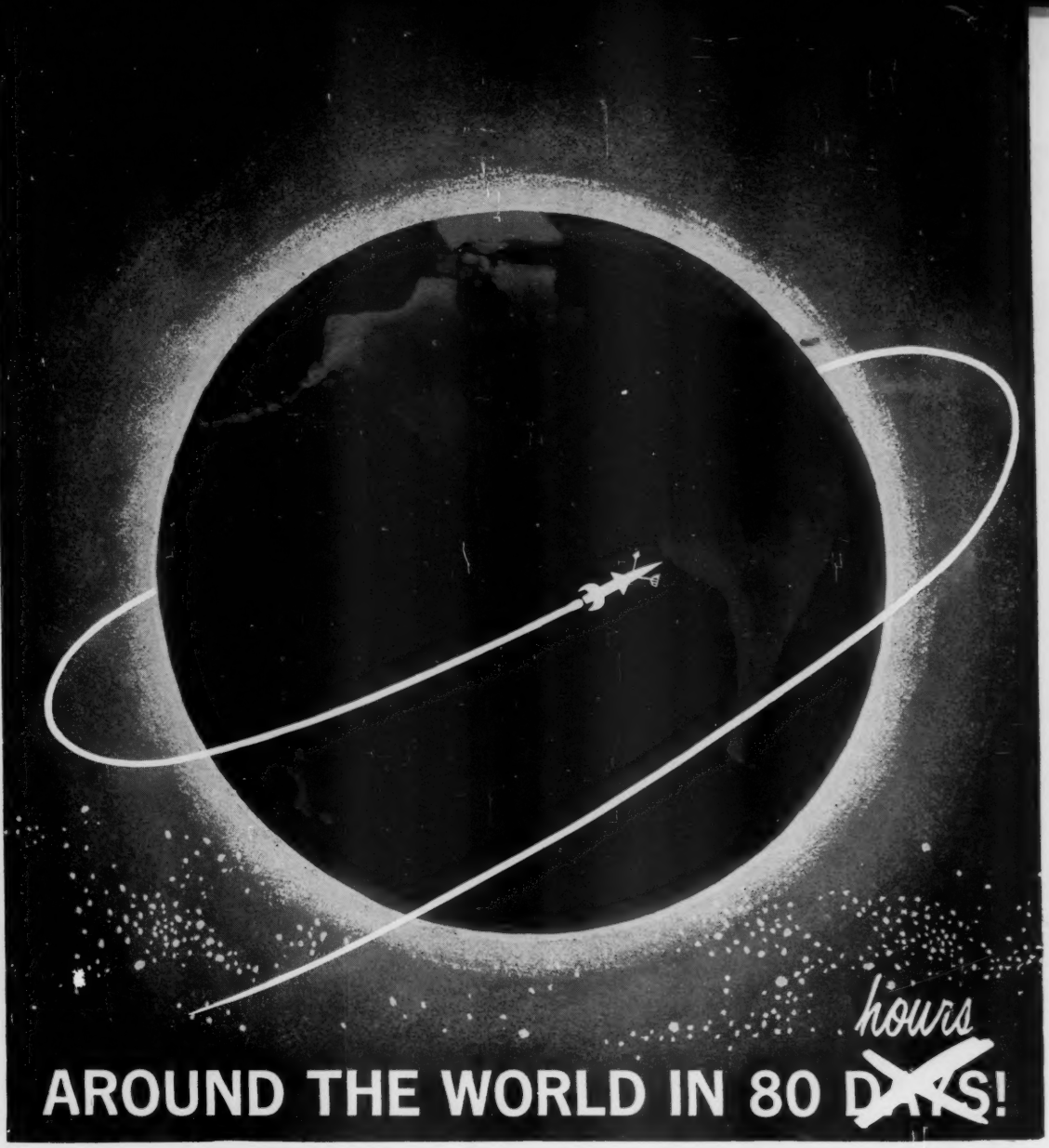
SCIENCE

1 April 1960
Vol. 131, No. 3405

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE





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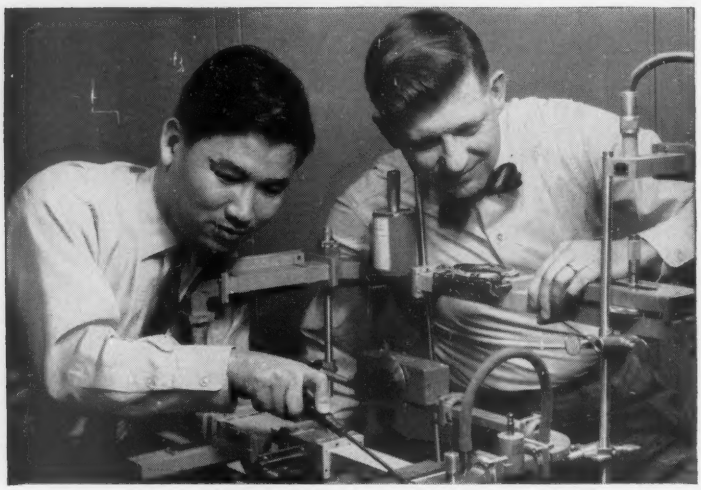
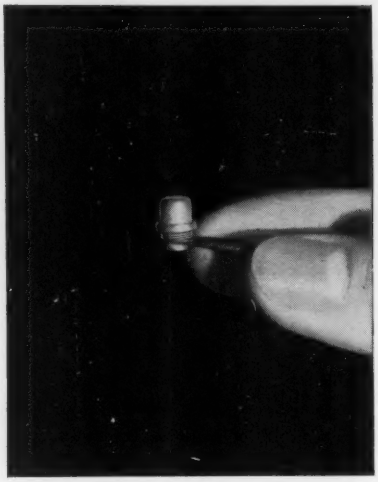
THE IDEA THAT GREW FOR 100 YEARS



At Bell Laboratories, M. Uenohara (left) adjusts his reactance amplifier, assisted by A. E. Bakanowski, who helped develop first suitable diode. Extremely low "noise" is achieved when certain diodes are cooled in liquid nitrogen.

First practical diode for amplifier, shown here held by tweezers, was jointly developed by A. E. Bakanowski and A. Uhlir.



How basic scientific ideas develop in the light of expanding knowledge is strikingly illustrated by the development of Bell Laboratories' new "parametric" or "reactance" amplifier.

Over 100 years ago, scientists experimenting with vibrating strings observed that vibrations could be amplified by giving them a push at strategic moments, using properly synchronized tuning forks. This is done in much the same way a child on a swing "pumps" in new energy by shifting his center of gravity in step with his motion.

At the turn of the century, scientists theorized that *electrical* vibrations, too, could be amplified by synchronously varying the *reactance* of an inductor or capacitor. Later amplifiers were made to work on this principle but none at microwave frequencies.

Then came the middle 50's. Bell Telephone Laboratories scientists, by applying their new transistor technology, developed semiconductor diodes of greatly improved capabilities. They determined theoretically *how* the electrical capacitance of these new diodes could be utilized to amplify at *microwave* frequencies. They created a new microwave amplifier with far less "noise" than conventional amplifiers.

The new reactance amplifier has a busy future in the battle with "noise." At present, it is being developed for applications in tropospheric transmission and radar. But it has many other possible applications, as well. It can be used, for instance, in the reception of signals reflected from satellites. It is still another example of the continuing efforts to improve your Bell System communications.

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Cover

An insect gained entrance to an agar plate inoculated at five sites with *Pasteurella pestis* and left a record of its travels. It probably entered through the chipped rim of the petri dish. [H. B. Levine]

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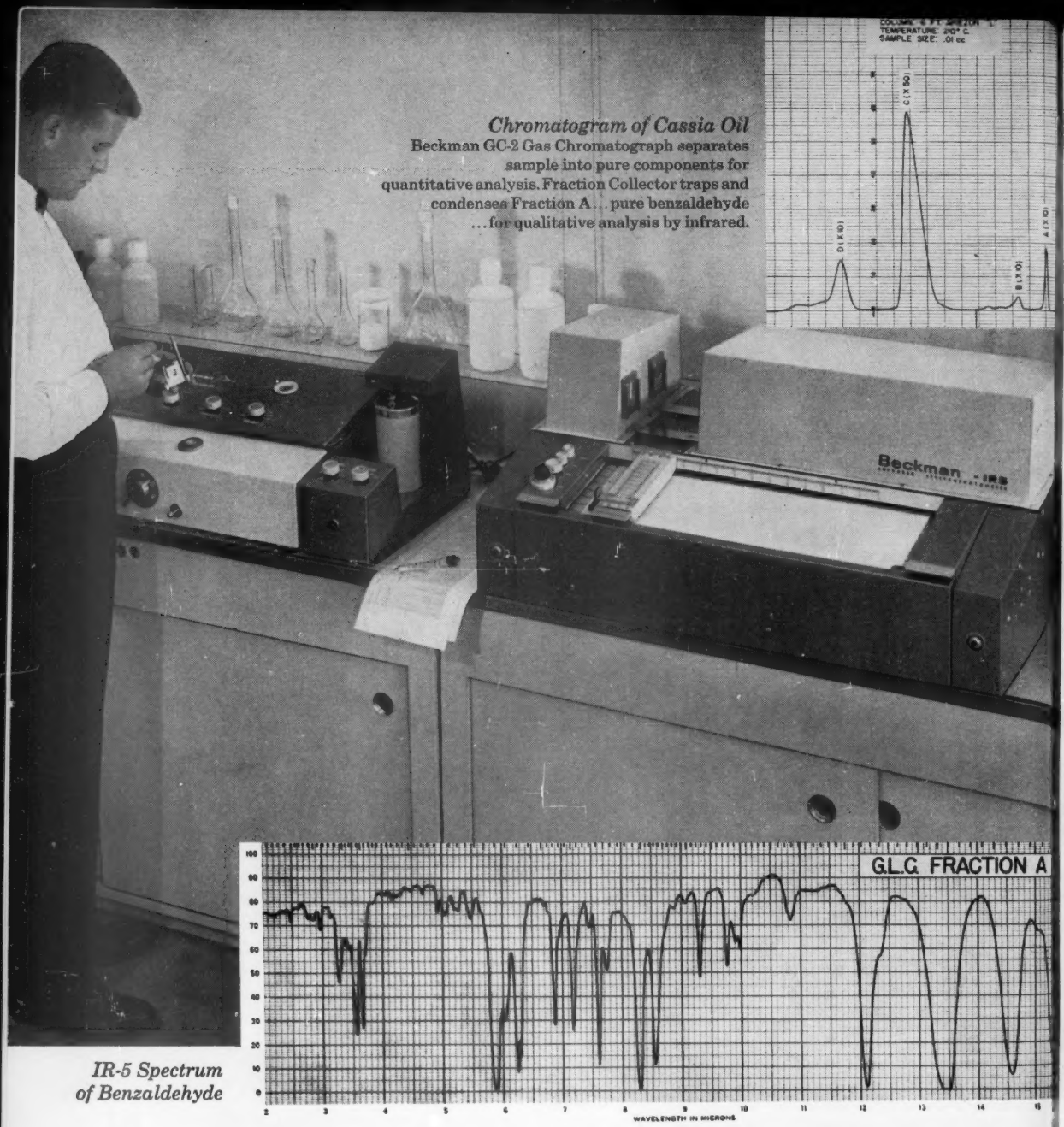

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Editorial and personnel-placement correspondence should be addressed to SCIENCE, 1515 Massachusetts Ave., NW, Washington 5, D.C. Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of manuscripts or for the opinions expressed by contributors. For detailed suggestions on the preparation of manuscripts and illustrations, see *Science* **125**, 16 (4 Jan. 1957).

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The Florence Agreement

In 1948 the policy-making body of the United Nations Educational, Scientific and Cultural Organization decided to seek an international agreement to promote the flow of cultural and educational materials among nations. Representatives of 25 member states drafted an instrument for this purpose which was unanimously adopted at a meeting of the General Conference of UNESCO in Florence in July 1950. This Agreement on the Importation of Educational, Scientific, and Cultural Materials (commonly referred to as the Florence Agreement) became operative in 1952, when it had been ratified by ten member states.

The central feature of the agreement is that certain materials will be exempt from customs duties: books, documents, articles for the blind, and, if they are of an "educational, scientific or cultural character," works of art, visual and auditory materials, and scientific instruments or apparatus.

In October 1957 representatives of 52 nations attended a UNESCO meeting held to appraise the first five years of operation of the agreement. They concluded that the agreement was "a valuable and, on the whole, highly effective instrument" and recommended that all states "should apply it and in the most liberal manner."

The United States, although it has been a party to the negotiations since the beginning, has been slow to move toward adherence. Finally, on 24 June 1959, the U.S. became the 32nd nation to sign the agreement. This action was ratified by the Senate by a 76 to 14 vote on 23 February 1960.

But one last hurdle remains. Since the agreement proposes removal of tariffs, the enabling legislation must be introduced in the House of Representatives. A bill is now being prepared by the Executive branch (the delay is puzzling since the State Department might well have begun preparation of appropriate legislation last June) which should be ready for introduction in the House within the next few weeks.

In the House the bill will be assigned to the Ways and Means Committee. Those in favor of the bill—the American Council on Education, the American Council of Learned Societies, the American Library Association, the American Book Publishers Council, and the American Association of Physics Teachers—will continue to testify in its support. The only group opposed prior to Senate ratification was the Scientific Apparatus Makers Association, which feared that a liberal interpretation and administration of the agreement might damage their industry. This position seems unrealistic, but will doubtless be put forward again. The only instruments that will be duty-free are those that are purchased by educational and scientific research institutions, and then only if "instruments or apparatus of equivalent scientific value are not being manufactured in the country of importation." Thus, most scientific instruments will not qualify. Furthermore, to facilitate U.S. participation, a special reservation was added, stating in effect that if any product is being imported in such relatively increased quantities as to threaten serious injury to the industry producing competitive products, "the contracting State shall be free . . . to suspend in whole or in part, any obligation under the Agreement with respect to such product."

These provisions give adequate—some would say more than adequate—protection to our instrument makers. It is to be hoped that, in the rush to adjournment prior to the party conventions, this bill, which makes a small but significant contribution toward breaking down international barriers and (to quote President Eisenhower) creating "a world community of open societies," will not die in committee.—G.DuS.

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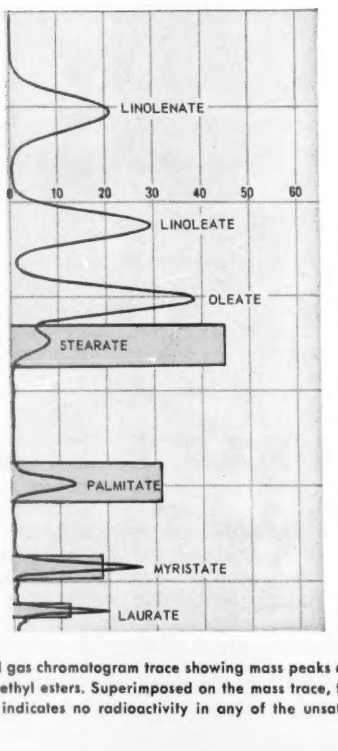

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CURRENT PROBLEMS IN RESEARCH

Low-Velocity Layers in the Earth, Ocean, and Atmosphere

These layers increase the difficulty of locating buried explosions and may cause sonar booms.

Beno Gutenberg

The most important "law" pertaining to the propagation of elastic waves (sometimes called sound waves) through solids, liquids, or gases is Fermat's principle. This states that the waves follow a path which makes their transmission time between two points a minimum. From this principle it follows that, along a given ray in the earth,

$$(r \sin i)/V$$

is constant, where r is the distance of a given point from the earth's center, V is the wave velocity at the point, and i is the angle between ray and radius. If the velocity V increases with depth, i also increases, and the ray is concave downward (Fig. 1*a*); similarly, if V decreases with depth, the ray is concave upward. In this case there are two possibilities. If in a layer the rate of decrease dV/dr is smaller than V/r, the radius of curvature of the ray is larger than that of the sphere through the point, and the ray returns to the surface (Fig. 1*b*); if the rate is larger, the ray goes downward (Fig. 1*c*). Near the earth's surface, V/r is close to zero.

Combination of layers with increasing and decreasing velocities (Fig. 2) leads to complicated wave paths which may produce shadow zones. These are entered only by diffracted energy or other types of waves. The time-distance curves (Fig. 2, right) show that direct waves arriving beyond the shadow zone are delayed relative to those arriving at short distances. The shadow zones as well as the time-distance curves change with the depth of the source (H in Fig. 2). If the source is in a layer with relatively low velocity (Fig. 2*d*), waves leaving the source nearly horizontally may travel continuously up and down without ever reaching the surface and may form a "channel." On the other hand, waves arriving just beyond the shadow zone at the surface concentrate and form a "caustic" (see Fig. 2). In the neighborhood of caustics the arriving waves are especially large. Similar phenomena occur if the decrease in velocity is sudden at a discontinuity.

Since shadow zones are the main result of low-velocity layers, these layers may easily be overlooked, especially if waves arriving slightly later than the direct waves under consideration are incorrectly assumed to be direct waves arriving in the actual shadow zone. Thus, investigations of shadow zones are usually difficult, and frequently there is no proof that a low-velocity layer exists. Consequently, many results concerning low-velocity layers in the earth have been the subject of controversy for years. However, there is no reasonable doubt that there are low-velocity layers in the solid earth, the ocean, and the atmosphere.

The late Dr. Gutenberg was director of the Seismological Laboratory, California Institute of Technology, Pasadena.

The Earth's Core

The portion of the earth first found to be a low-velocity layer was the outer portion of the core. Oldham had found in 1906, and Wiechert in 1907, that longitudinal waves through the deep interior of the earth are delayed, the delay indicating relatively low velocity somewhere deep in the earth. In 1913 I realized that the travel-time curve of these waves has the form shown in Fig. 2, *a* or *b*: There is a shadow zone, starting at a distance slightly more than halfway around the earth from the source, and at the distance where the delayed waves arrive, more than three-quarters of the way around the earth from the source, they are extremely strong, indicating a caustic. On the basis of these observations I concluded in 1913 that the core has a radius of 2150 miles (3470 kilometers) and that the wave velocity decreases at its boundary from 8.2 to 5.3 mi/sec (from 13.15 to 8.5 km/sec); the velocity outside the core boundary is now believed to be about 0.3 mi/sec more, that inside the core boundary about 0.3 mi/sec less; the figure for the radius of the core which is being used at present is the same as the earlier figure, within a few miles.

The relatively great decrease in the wave velocity at the core boundary is believed to be caused by the fact that the "mantle" of the earth surrounding the core is solid, while at least the outer portion of the core is not solid. This had been concluded first from the tides of the earth's body, which are noticeably greater than they would be if the earth were solid and very rigid throughout. The core is much less rigid than the mantle. No transverse waves which had been propagated through the core have been found. The small rigidity of the core results in longitudinal waves of relatively low

velocity. Another explanation for the decrease in the wave velocity at the core boundary is the probable sudden increase in density at the core boundary. Contrary to a frequently held belief, greater density results in lower wave velocity.

Asthenosphere Channel

In 1926 I made an investigation to determine whether the amplitudes of waves through the upper portion of the earth give any indication of whether or not the melting point of the material is reached somewhere near a depth of 50 miles, as had been expected by some volcanologists and geologists. I found that the amplitudes of longitudinal waves arriving at a distance of 1000 miles from the source of earthquakes are only about 1/100 of those arriving at a distance of 100 miles, and that this decrease in amplitude is gradual. At a distance of roughly 1000 miles the amplitudes increase very rapidly, to about the values which they have only 100 miles from the source. This I interpreted as an indication of a wave velocity at a depth of about 50 miles that was relatively low but not low enough to indicate a liquid material. This low-velocity layer and the consequences of its presence have been the subject of investigations ever since.

About the turn of the present century, it was concluded that near a depth of roughly 100 miles below the earth's surface there must be a layer which yields more easily to stresses of secular duration than the stronger crustal layers and permits the slow, gradual movements required to explain various observations. For example, accumulation or removal of loads at the surface of the earth, especially of ice during ice ages, is followed by gradual sinking or uplift, respectively, of the affected areas during the following centuries or even millennia; moreover, except for geologically disturbed areas, the earth's crust is nearly in equilibrium (isostasy) regardless of the elevation of the surface (mountains and oceans). These and other findings have been considered to be an indication that a layer exists below the crust in which a slow flow of the material is possible. This relatively "weak" layer has been called the "asthenosphere" since 1914. I have believed that it coincides with the asthenosphere low-velocity channel indicated by the seismic waves ever since I found the channel in 1926.

Results Based on Deep-Focus Earthquakes

Figure 2 shows that the shadow zone decreases in size as the source H moves toward the bottom of the low-velocity channel (Fig. 2e); no shadow zone should exist if the source is still deeper. Consequently, I decided, jointly with C. F. Richter, that a study of the records of earthquakes to compare the behavior of amplitudes of shocks originating at various depths should give some information about the depth of the channel. We found that in Peru the shadow zone for longitudinal waves disappears when the focal depth of the shocks exceeds about 100 miles. Later, I studied for different regions the amplitudes of waves recorded at various epicentral distances in deep-focus earthquakes in connection with the problem of developing graphs for finding the magnitude of deep shocks. Figure 3 is based on the results of these investigations. It shows that the minimum amplitude of longitudinal waves increases as the focal depth increases, and that, practically, the minimum is absent if the source is at depths in excess of about 150 miles. For transverse waves the shadow zone extends to slightly greater distances but is otherwise similar.

Results Based on Records of California Shocks

In Fig. 4, the beginnings of selected records obtained from the Kern County, California, earthquake of 21 July 1952 are reproduced. The first, recorded at an angular distance of 3.9° (269 miles along the earth's surface) shows a rather large, sharp beginning, produced by a longitudinal wave (a in the figure) which had been refracted (or diffracted) at the Mohorovičić discontinuity, which separates crust and "mantle," but had not penetrated far into the mantle. This wave is much smaller at a distance of 6.3° (435 miles) and has completely disappeared at a distance of 7.4° (511 miles); the latter point, consequently, is in the shadow zone. It is obvious that a later wave (b) may be mistaken for the missing wave (a), and the shadow zone may be missed. The next two seismograms in Fig. 4 (for distance of 10.1° and 12.9° illustrate a similar situation. The seismogram (Fig. 4, middle) which first shows the relatively large beginning (c) of the delayed second branch near its caustic beyond the shadow zone was recorded at a distance of 15.2° (1050 miles). The last records, from distances of 18.6° and 19.3° (1283 and 1332 miles) respectively, still have rather large beginnings. The explanation of travel-time curves for earthquakes at epicentral distances between about 400 and 2000 miles is still a matter of controversy. However, the low-velocity layer is now very widely considered to be the cause of the complications.

In 1953 I pointed out that the apparent velocity (measured along the earth's surface) which the travel-time curve exhibits at its only point of inflection can be used to determine the velocity at the depth of the source for each given earthquake. Theoretically, this point of inflection corresponds to the ray which leaves the source horizontally, and the corresponding apparent velocity at the surface has its minimum. Even if there is a shadow zone, this minimum apparent velocity can be found with good approximation. If multiplied by the ratio of the radius at the source to the earth's radius, it gives the true wave velocity at the depth of the source.

In 1959 I studied information on the velocities immediately below the crust. The waves used for this purpose correspond to the beginning (a) of the first and second seismograms in Fig. 4. On the basis of results published by seismologists in many regions for shallow earthquakes and for artificial explosions, I found that, under continents, the following relationship exists: the deeper the extension of the crust into the mantle below—that is, the deeper the Mohorovičić discontinuity—the lower the velocity immediately be-

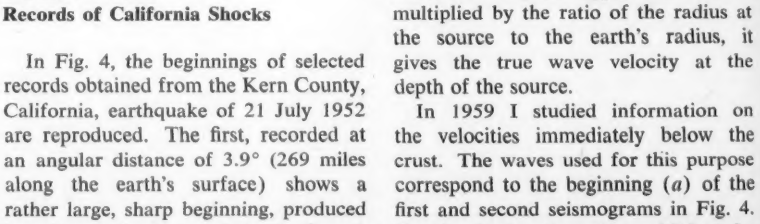


Fig. 1. Sound rays (schematic): (a) if the velocity V increases with depth h; (b and c) if it decreases; (b) if dV/dh is smaller than V/h; (c) if dV/dh is greater than V/h.

low the crust. Below continents, this depth varies between about 15 and 40 miles. Consequently, I concluded that immediately below the crust the velocity of both wave types decreases with increasing depth. I found that, on the average, this decrease exceeds the critical value $dV/dr = V/r$ mentioned at the beginning of this article. Thus, under average continents, the asthenosphere low-velocity channel begins at the bottom of the crust. In Fig. 5 my most recent findings for the velocities V of longitudinal, and v of transverse, waves in the upper portion of the mantle are reproduced. They show that the low-velocity channel for transverse waves extends to a greater depth than that for longitudinal waves, and that it has a minimum at a depth of almost 100 miles, while the minimum velocity of longitudinal waves in the earth's mantle is closer to 50 miles.

During the past two years additional information on the wave velocities as a function of depth has been gained from investigations of the dispersion of surface waves. There are two types of such waves, Love waves and Rayleigh waves. Their periods in records of distant earthquakes vary usually between about 10 and more than 100 seconds, and their velocities vary between about 2 and 2.75 mi/sec; consequently, most wavelengths are in the range between roughly 20 and more than 300 miles. Since most of the energy of surface waves is propagated within one wavelength below the earth's surface, the velocity of the longer waves is affected by much deeper portions of the earth's mantle than that of shorter waves. It is possible to calculate the velocities of surface waves if the velocities of longitudinal and transverse waves as a function of depth are given.

Until recently such calculations were very tedious, and not more than three layers could be assumed. With modern computers it is now possible to perform the calculation in a relatively short time, on the assumption of different velocities in 20 or more layers. Several groups of seismologists have performed such calculations of the velocities for both types of surface waves and have found that the calculated and observed dispersion curves agree best if an asthenosphere low-velocity channel of approximately the type given by the curves in Fig. 5 is assumed. This channel seems to be thicker under ocean bottoms than under continents. This results from the fact that the crust,

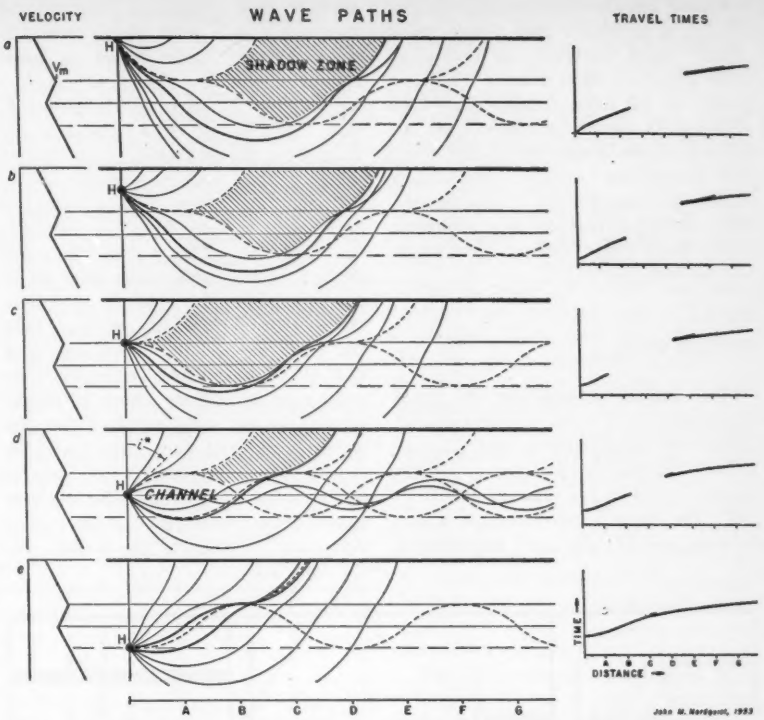


Fig. 2. Paths of sound waves (seismic waves), if the velocity changes with depth, as given at left. (Center) Wave paths for various depths of the source H. (Right) Corresponding transmission times as a function of the distance from the source.

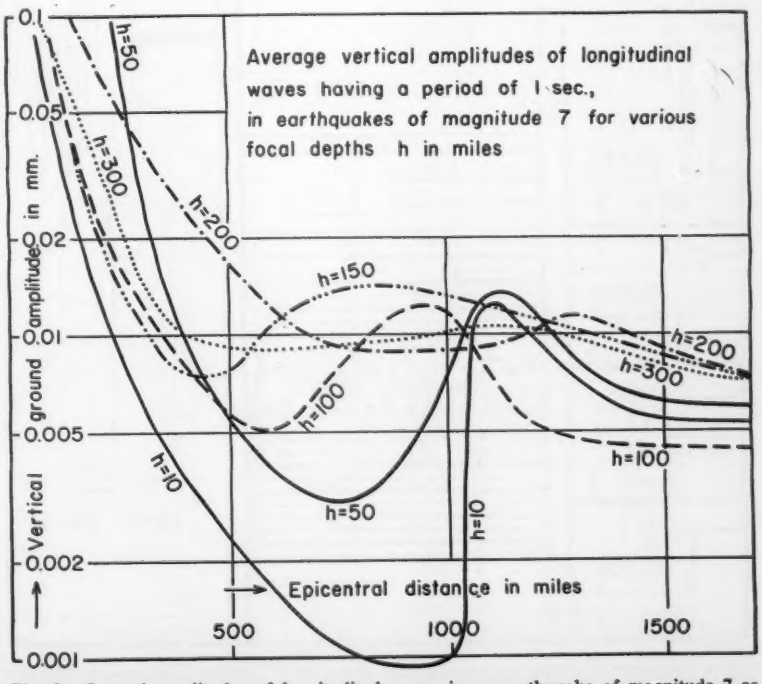


Fig. 3. Ground amplitudes of longitudinal waves in an earthquake of magnitude 7 as a function of the epicentral distance for various depths of the source; it is assumed that the wave periods are 1 second.

bounded by the Mohorovičić discontinuity, is frequently only 5 miles thick under ocean bottoms, as contrasted with about 15 miles (under some low lands) to 40 miles (under some high mountains) under continents. As we have seen, the deeper the discontinuity, the lower the velocity immediately under the discontinuity. Consequently, this velocity is reached at a shallower depth under high continents than under oceans (see Fig. 5).

Channel Waves

The Italian seismologist P. Caloi concluded in 1953 that "channel waves" should be propagated in the asthenosphere channel if the source of an earthquake is in the channel (see Fig. 2*d*). He found many instances where seismograms of deep-focus earthquakes showed such waves as long as the source was at a depth of not over 150 miles. These waves travel to great distances, with a velocity of about 5 mi/sec (8 km/sec) for longitudinal, and 2.75 mi/sec (4.4 km/sec) for transverse, channel waves, regardless of whether they are propagated under continents or oceans. These velocities correspond to those found in the channel (Fig. 5). The waves affect the surface of the earth, since they travel only between one and a very few wavelengths below the surface. On the other hand, it has been suggested that all channel waves in or near the crust may be surface waves of higher modes. In any case, the fact that the waves considered here have been observed along continental and oceanic paths shows that the asthenosphere channel is not interrupted at the transition from continental to oceanic regions. Such waves were observed independently in 1954 by Press and Ewing and later by others. In all instances, the depth ranges of the earthquakes involved and the velocities observed were the same, within small limits of error.

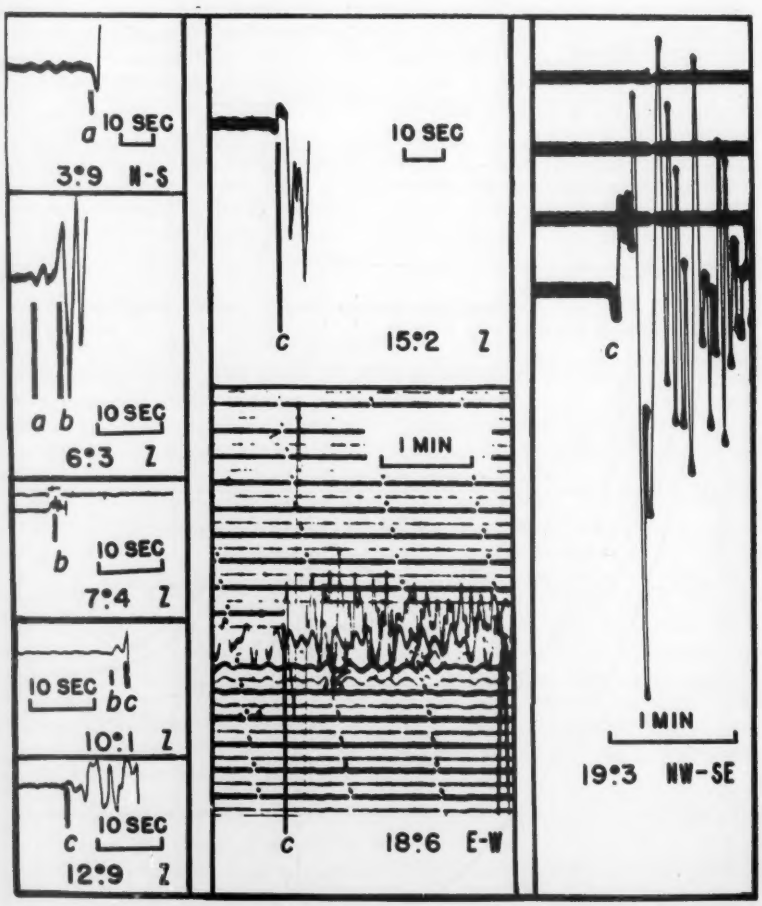


Fig. 4. Beginnings of selected seismograms of the Kern County, California, earthquake of 21 July 1952 (1° = 69 miles). (*N–S*) The north-south component; (*Z*) the vertical component; (*E–W*) the east-west component; (*NW–SE*) a record obtained in the direction northwest-southeast.

Cause of the Asthenosphere Channel

When I found the first indication of the asthenosphere channel in 1926 I was trying to determine whether there is any indication that molten material is present at a depth of 50 to 100 miles, where the estimated temperatures are close to the estimated melting point of the rocks expected there. The most recent estimates of the temperature at a depth of 60 miles vary between about 800° and 1500°C, those of temperatures at a depth of 180 miles, between about 1400° and 2000°C, while the corresponding melting points are estimated very roughly to be at 1500° and 1900°C, respectively. Thus, all we can say at present is that the melting point is fairly close to the actual temperature. However, at depths below 500 miles, practically all the estimated temperatures in the mantle are well below the estimated melting points.

A very interesting contribution to the questions of whether the melting point is reached in the upper mantle, and if so, where, was furnished by the Russian volcanologist G. S. Gorshkov in 1957. He found that no transverse waves are recorded in Kamchatka from Japanese earthquakes when a portion of their path follows the volcanic belt between Japan and Kamchatka, while they are well recorded at stations in Kamchatka to which the wave paths are only slightly different but not under the volcanic chain. Gorshkov concluded that there are foci of nonsolid magma at a depth of about 35 miles under the volcanoes—that is, at a depth similar to that of the asthenosphere channel.

Laboratory experiments to find the velocities of waves in rocks at the temperatures and pressures in the earth's crust are not decisive. The conclusions depend on assumptions concerning the change in temperature with depth in the earth which are uncertain, as explained above. However, the results of the experiments do not exclude a decrease in velocity with depth in the asthenosphere channel. I believe that in the near future attempts should be made to

reverse the calculations—that is, to assume values for the change of the wave velocities as a function of depth (for example, on the basis of data in Fig. 5) and to calculate the corresponding temperature in the upper portion of the mantle. A serious problem in all such calculations is that of deciding which rock type should be assumed.

The shadow zone produced by the asthenosphere channel (see in Fig. 3 the curve for a focal depth of 10 miles) has played an important role in the international discussions of the problem of how many stations are needed to locate atomic subsurface explosions. It has been realized during the discussions that stations have to be either fairly close to the source or over 1000 miles away. Figure 4 gives an idea of the differences in the amplitudes involved.

Lithosphere Low-Velocity Channels

Seismograms showing waves which have been transmitted only through the various layers of the earth's crust are rather complicated. About ten years ago it appeared that data from earthquake records were leading to different results, for the velocities involved, from data from artificial explosions. When I found, in addition, that the amplitudes of waves through a given layer may decrease faster with distance than would be expected if the velocity increases with depth, I concluded, in 1951, that in portions of at least two crustal layers the velocity may decrease with depth. Detailed investigation of these "lithosphere channels" is more difficult than that of the asthenosphere channel, since local differences in crustal structure are greater in the crust than in the upper mantle, and it is frequently difficult to identify correctly the travel-time curve for waves through a given layer.

A break in this research came when Press and Ewing in 1952 discovered waves of two new types which travel with constant velocity. These waves have been observed only along continental paths. These, as well as additional waves of similar type, were investigated soon afterwards by Miss Lehmann in Denmark and M. Båth in Sweden. Båth established the fact that these waves disappear not only at the transition from continent to ocean but also under large mountain chains where the crustal layers are disturbed. These waves usually appear as a very striking short-period motion riding on top of the earliest, appreciably longer, surface waves. The regular "microseisms" which are visible continuously on records of sensitive instruments at most stations and are produced by high ocean waves near the coast may be lithosphere channel waves.

The cause of the lithosphere low-velocity layers is probably, again, the increase in temperature with depth. Close to the surface the effects of the increase in pressure (the closing of pores) prevail; below a depth of a few miles the effects of the increase in temperature may surpass those of the increase in pressure. Again, laboratory experiments are not decisive, for the same reasons which were cited in connection with the asthenosphere channel.

In the sedimentary layers of the crust, where the search for oil and minerals depends much on seismic methods, there are many "low-velocity" layers which interfere with the investigations of the structures still more than those in the deeper portion of the crust. For example, two types of layers may alternate with increasing depth; the one with the lower velocity creates a low-velocity layer each time it occurs. The resulting shadow zones are frequently a source of incorrect conclusions. Exploding the charge in a low-velocity layer may result in poor records.

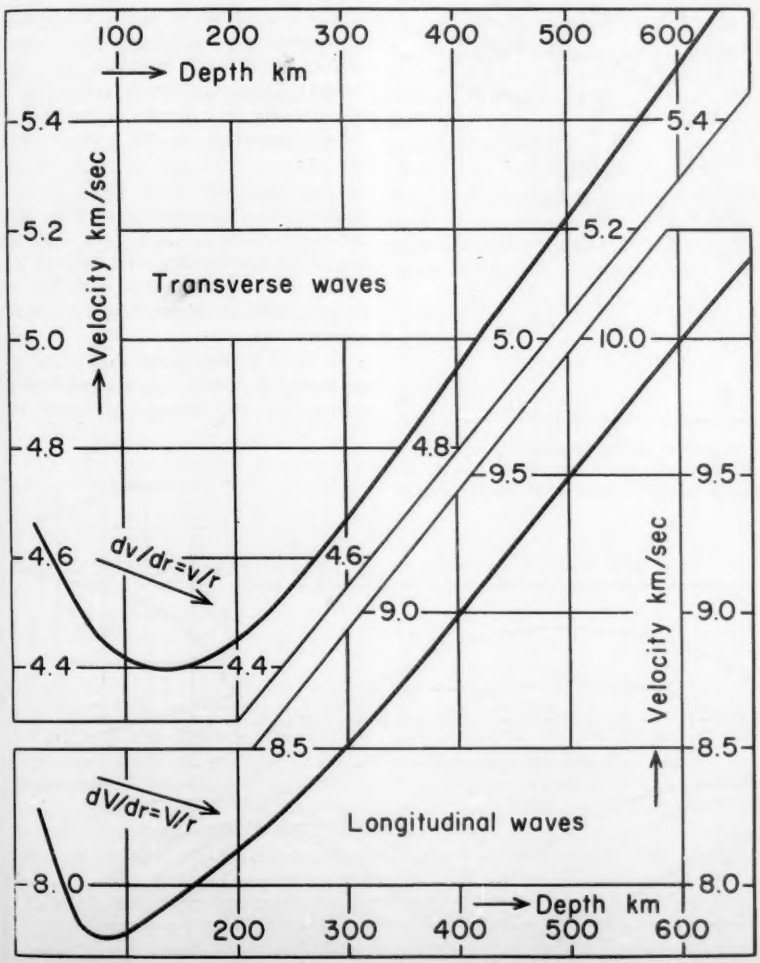


Fig. 5. Velocities V of longitudinal, and v of transverse, waves (in kilometers per second) as a function of the depth in the earth (in kilometers).

Low-Velocity Channel in the Ocean

The velocity of sound waves in the ocean increases with increasing temperature, salinity, and pressure of the water. Usually the rather rapid decrease in temperature in the upper layers of the ocean results in a decrease in sound

velocity with depth, while deeper down the effect of the increase in pressure prevails. Near the surface the sound velocity is nearly 5000 ft/sec (it depends on temperature and salinity), and it decreases usually by a few percent to a minimum at a depth of less than a mile. Thus, we must normally expect in the ocean a low-velocity layer with all the phenomena indicated in Fig. 2. This conclusion was verified in 1934 by Dyk and Swainson of the United States Coast and Geodetic Survey, in little-publicized experiments. Independently, Ewing and Worzel concluded in 1948 that the sound of an explosion of 4 pounds of TNT could be identified at a distance of 10,000 miles, if the TNT was exploded at a depth of 4000 feet in the low-velocity layer (see Fig. 2*d*). On the other hand, no sound from a submarine near the point *H* in Fig. 2*a* is audible aboard a ship in the shadow zone, which, under the conditions in the ocean, may begin immediately at *H* and may extend for 50 or even more miles around *H*.

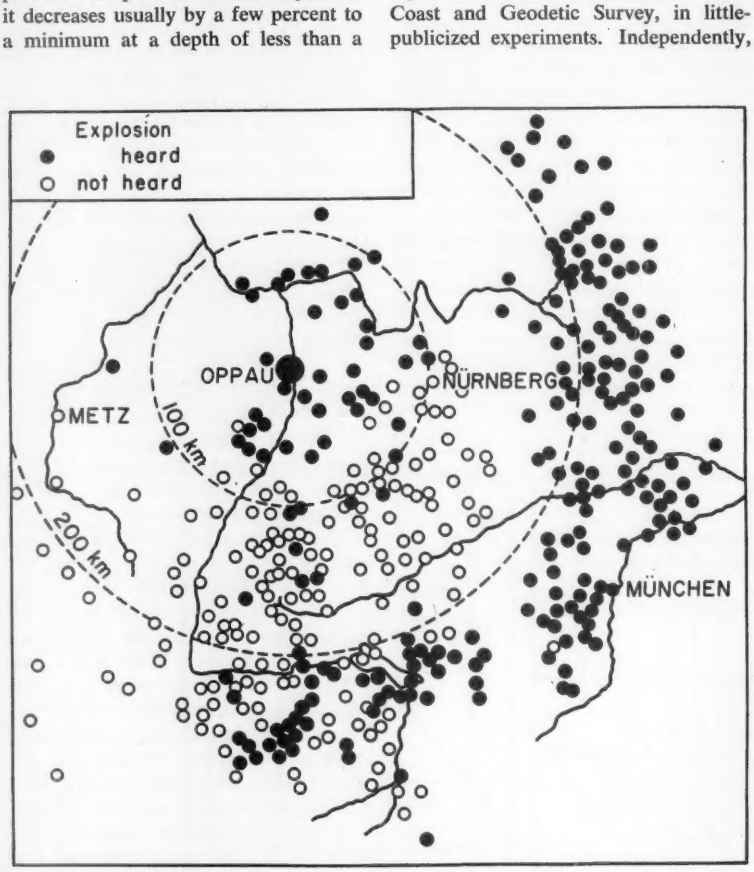


Fig. 6. Points at which the sound from the explosion at Oppau (Baden, Germany) on 21 September 1921 was heard and points where it was not heard (data from A. de Quervain). No observations were collected to the northwest and north of the source beyond a distance of about 100 km.

Low-Velocity Channels in the Atmosphere

In the atmosphere, we may assume with sufficient accuracy a constant composition for the lowest 50 miles. In this case the sound velocity is proportional to the square root of the absolute temperature of the air. Since this decreases throughout the troposphere, except for relatively thin inversion layers, the sound velocity decreases, too, to a height of roughly 5 miles, depending on the latitude and the season. Since the temperature increases again at higher levels in the stratosphere, low-velocity layers are formed and we can apply the data in Fig. 2, which we now have to look at upside down. The effect of wind has to be considered in interpreting the observations.

In 1903 it was found that after an accidental explosion a zone of audibility surrounded the source, a "zone of

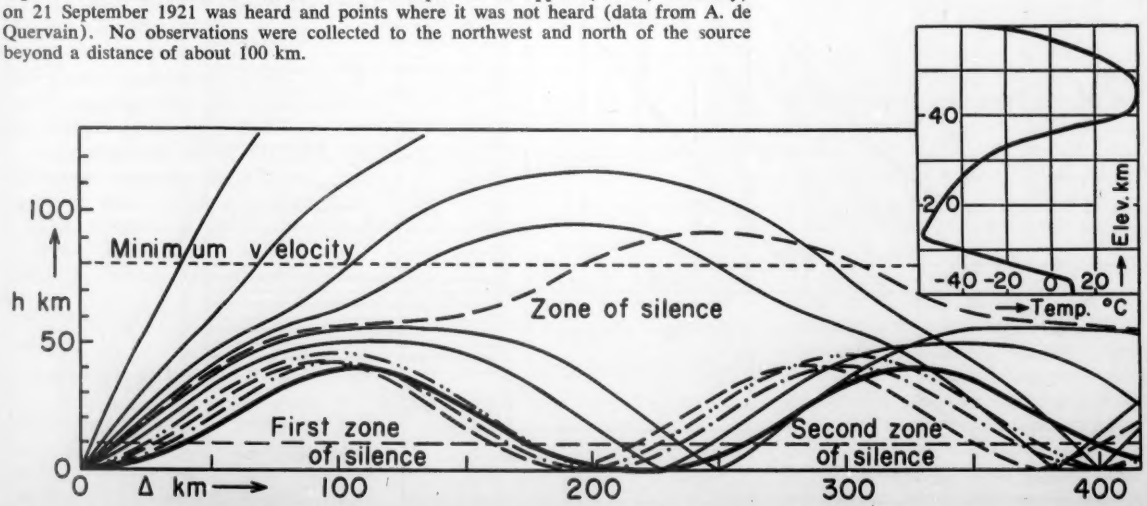


Fig. 7. Average paths of sound waves in the atmosphere [Δ, distance; *h*, elevation above ground (both in kilometers)]. (Insert) Characteristic temperature (in degrees centigrade) in middle latitudes as a function of the elevation *h*.

silence" followed, and at a distance of roughly 100 miles there was again a zone of strong sound. Figure 6 shows similar observations collected, after an accidental explosion in 1921, by the Swiss seismologist de Quervain. Richter and I reported an instance in 1930 in which target practice of the Navy off southern California was heard near the source; then came a zone of silence; then, at distances of about 70 miles from the source, the sound was so strong that windows rattled severely and, in one case, were even cracked. There is little doubt that sonic booms which sometimes irritate the population over fairly large areas may result from planes accelerating beyond the sound barrier; these planes may well be between 50 and 100 miles away (see, in Fig. 2, the caustic at the end of the shadow zone, and see Fig. 7). These large amplitudes correspond to those recorded in earthquakes about 1000 miles from the source (Fig. 4, middle and right).

Details of the sound propagation in the atmosphere were first investigated about 30 years ago, when the temperature in the atmosphere above an elevation of about 15 or 20 miles was still unknown. Discovery of the existence of the caustics beyond the zone of silence (Fig. 6) and the observed travel times of the sound left no doubt that at greater elevations the sound velocity, and consequently the temperature, must increase again. Figure 7 shows results for the temperature and wave paths based on calculations which I made in 1925. At that time, great doubt was expressed about the calculated relatively high temperature at an elevation of about 30 miles. Recent direct observations show that the temperatures calculated from the observed sound waves arriving at the ground were substantially correct.

In 1949 Cox pointed out that a second minimum of the temperature and the sound velocity at an elevation of about 50 miles produces a second low-velocity layer. Its effect on the wave paths is indicated in Fig. 7. However, the corresponding waves arriving at the earth's surface are relatively small, since at elevations above 50 miles the absorption of sound increases rather rapidly; the molecules are too far apart to permit good transmission of energy.

Finally, sound waves have been observed to circle the earth repeatedly after large explosions—for example, that of Krakatoa or those of atomic blasts in the atmosphere. These waves correspond in principle to the channel waves (Fig. 2*d*), although other atmospheric processes affect the details.

We thus find that one single, little-publicized phenomenon—the low-velocity layer—which has been disregarded by some geophysicists and mentioned infrequently by others in explanation of observations, plays an important role in many instances of wave propagation through the earth's solid body, the oceans, and the atmosphere (*1*).

Note

1. I presented the first version of this article, "Low-velocity layers in the earth's interior, the ocean and the atmosphere," upon invitation at a meeting of the American Physical Society in Mexico City in June 1950. A revised version was my presidential address before the International Association of Seismology and Physics of the Earth's Interior, in Rome, 14 Sept. 1954. This was published in condensed form in *Geofisica pura e applicata* [**28**, 1 (1954)]. This version contains many references and the original of Fig. 2. The present version is contribution No. 941 of the Division of the Geological Sciences, California Institute of Technology, Pasadena. More detailed data on the low-velocity layers in the earth, and on temperature and pressure and other physical conditions in the earth, may be found in B. Gutenberg, *Physics of the Earth's Interior* (Academic Press, New York, 1959). Additional data are given in B. Gutenberg, "The asthenosphere low-velocity layer," *Annali di Geofisica* (in press). For data concerning the low-velocity layer in the ocean, see M. Ewing and J. L. Worzel, "Long range sound transmission," in "Propagation of Sound in the Ocean," [*Geol. Soc. Am. Mem. No. 27* (1948)]; also, K. Dyk and O. W. Swainson, "The velocity and ray paths of sound waves in deep sea water," *Geophysics* **18** (1953). For discussion of propagation of sound in the atmosphere, see B. Gutenberg, in *Compendium of Meteorology* (American Meteorological Society, 1951), pp. 366–375.

Beno Gutenberg, Geophysicist

The death of Beno Gutenberg in Pasadena, California, on 25 January 1960 marked the end of an era in seismology. That era had been dominated by him. A prodigious worker with broad interests, his was the era before computing machines and team attacks.

Working alone and with his colleague Charles Richter, he covered the gamut of seismology. His early interest in meteorology continued throughout his life, as did his broad interest in the general problems of the physics of the earth.

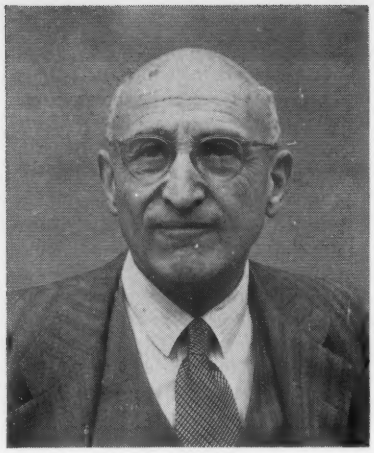
Beno was born in Darmstadt in Hesse on 4 June 1889. As a student in the University of Göttingen he worked under Emil Wiechert. At the age of 22 he received the degree of doctor of philosophy, with a thesis on the subject of microseisms. For a year after receiving his degree he remained in Göttingen and completed his great work of computing the depth of the earth's core. His value of 2900 kilometers still stands—one of the few numbers in seismology which is generally agreed upon.

Gutenberg served a year as meteorologist in the army. He then served on the staff of the International Seismological Central Station in Strasbourg. He was a professor at Frankfort in 1930 when he was called to California Institute of Technology as professor of geophysics and meteorology. He later was appointed the first director of the Seismological Laboratory, a position which he occupied until his retirement in 1957.

It is difficult to select particular items from the contributions of so prolific a researcher. The several editions of the *Seismicity of the Earth* (which he wrote with Richter) are a monumental work of great value. When Beno received in 1953 the Bowie medal of the American Geophysical Union the president referred to this work as the "Gutenberg Bible." His collaboration with Richter

in the development of the magnitude scale was most fruitful. His computations of energy release by earthquakes were well summarized in his William-Smith lecture before the Geological Society (London) in 1955.

Gutenberg's particular discovery was the low-velocity layer in the earth just below the Mohorovičič discontinuity. His conclusion that this layer exists shows his remarkable feeling for seismograms. The effect of the layer on the amplitudes of *P* waves in earthquakes was apparent to him but not to others. He would reduce the records of various seismographs from various earthquakes to a common denominator. For 30 years, almost alone, he maintained the existence of this layer. Fortunately, in the last two years before his death he found his view generally accepted on the grounds of records of explosions recorded on similar instruments and of the effect of the layer on the dispersion of surface waves. We now have the Gutenberg low-velocity layer as well as the Gutenberg discontinuity at the core boundary.



Beno Gutenberg

Gutenberg's work was recognized as widely abroad as at home. He received the Prix de Physique du Globe (1952) from the Académie royale de Belgique. He was a member not only of our National Academy of Sciences but also of the academies of New Zealand, Finland, Sweden, and Rome. He had been president of the International Association of Seismology and the Physics of the Earth's Interior as well as of the Seismological Society of America.

It is fortunate for us that Gutenberg had just published his last book, *Physics of the Earth's Interior*, before he died. It leaves to young geophysicists an account of earth physics, with particular emphasis on problems needing further study.

The affection which Gutenberg's family felt for him was very strong. He reciprocated fully. He is survived by his widow Hertha, by his son Arthur, and by his daughter Stefanie.

PERRY BYERLY
University of California, Berkeley

The World into Which Darwin Led Us

The Darwinian revolution changed the most crucial element in man's world—his concept of himself.

George Gaylord Simpson

Almost everyone is aware that the year 1959 was the centennial of the publication of *The Origin of Species* by Charles Darwin. It was also the sesquicentennial of Darwin's birth and, coincidentally, of the publication of *Philosophie Zoologique* by Lamarck, the first really important work on organic evolution. That sesquicentennial has been little noted, but the centennial has been most elaborately celebrated by conferences, symposia, all manner of meetings and oratory, and a veritable spate of publications. Every aspect of Darwin, his contemporaries, and his predecessors has been presented. The gamut runs from lavish eulogy of Darwin to peevish accusation of plagiarism and dishonesty. More responsibly, almost everything Darwin ever said or did has been carefully re-evaluated.

In the face of all these studies, it is now practically impossible to say anything fresh about Darwin. (I must confess to a growing surfeit on that topic, approaching boredom.) Yet there are aspects of the subject of such transcendent importance that they bear frequent repetition. At this point there is reason for a summing up not so much about Darwin himself as about the continuing impact of the revolution of which he was the chief initiator.

It has often been said that Darwin changed the world. It has less often been made clear just what the change has been. Darwin did not—to his credit he did not—make any of the discoveries that have led to our present overwhelming physical peril. Most, although not quite all, of our technology would be the same if Darwin's work had not been done, by him or anyone else. Doubtless we would in that case still have our same traffic jams, horror movies, bubble gum, and other evidences of high civilization. The paraphernalia of civilization are, however, superficial. The influence of Darwin, or more broadly of the concept of evolution, has had effects more truly profound. It has literally led us into a different world.

How can that be? If evolution is true, it was as true before Darwin as it is today. The physical universe has not changed. But our human universes, the ones in which we really have our beings, depend at least as much on our inner perceptions as on the external, physical facts. That can be made evident by an elementary example. Suppose a stone is seen by a small boy, an artist, and a petrologist. The small boy may per-

The author is Alexander Agassiz professor of vertebrate paleontology at Harvard University, Cambridge, Mass. This article is the text of an address presented 29 December 1959 at the annual meeting of the American Association for the Advancement of Science, in Chicago.

ceive it as something to throw, the artist as something to carve into sculpture, the petrologist as a mixture of minerals formed under certain conditions. The stone is three quite different things to the three people, and yet they are seeing exactly the same thing. The stone has identical properties whatever anyone thinks about it.

In that trivial example all three conceptions of the stone, although profoundly different, are equally true. The stone can indeed be thrown, be sculptured, or be analyzed petrologically by procedures suitable to each of the three perceptions. But there are differing perceptions of objects and of our whole world that are not equally true in the same sense, which is the scientific sense of material testability. Perceptions that are not materially testable or that have been contradicted by adequate tests are not rationally valid. As they petrify into tradition and dogma they become superstitions. Perception of the truth of evolution was an enormous stride from superstition to a rational universe.

The Changing Universe

Years ago I lived for a time with a group of uncivilized Indians in South America. Their world is very different from ours: in space, a saucer a few miles across; in time, from a few years to a few generations back into a misty past; in essence, lawless, unpredictable, and haunted. Anything might happen. The Kamarakoto Indians quite believe that animals become men and men become stones; for them there is neither limitation nor reason in the flux of nature. There is also a brooding evil in their world, a sense of wrongness and fatality that they call *kanaima* and see manifested in every unusual event and object.

That level of invalid perceptions might be called the lower superstition. It is nevertheless superior in some respects to the higher superstitions celebrated weekly in every hamlet of the United States. The legendary metamorphoses of my Indian friends are grossly naive, but they do postulate a kinship through all of nature. Above all, they are not guilty of teleology. It would never occur to them that the universe, so largely hostile, might have been created for their benefit.

It is quite wrong to think that uncivilized Indians are, by that token, primitive. Nevertheless, I suppose that the conceptual world of the Kamarakotos is more or less similar to that of ancient, truly primitive men. Indeed, even at the dawn of written history in the cradles of civilization, the accepted world pictures do not seem very different from that of those Indians.

The world in which modern, civilized men live has changed profoundly with increasingly rational, which is to say eventually scientific, consideration of the universe. The essential changes came first of all from the physical sciences and their forerunners. In space, the small saucer of the savage became a large disc, a globe, a planet in a solar system, which became one of many in our galaxy, which in turn became only one nebula in a cosmos containing uncounted billions of them. The astronomers have finally located us on an insignificant mote in an incomprehensible vastness—surely a world awesomely different from that in which our ancestors lived not many generations ago.

As astronomy made the universe immense, physics itself and related physical sciences made it lawful. Physical effects have physical causes, and the relationship is such that when causes are adequately known effects can be reliably predicted. We no longer live in a capricious world. We may expect the universe to deal consistently, even if not fairly, with us. If the unusual happens, we need no longer blame *kanaima* (or a whimsical god or devil) but may look confidently for an unusual or hitherto unknown physical cause. That is, perhaps, an act of faith, but it is not superstition. Unlike recourse to the supernatural, it is validated by thousands of successful searches for verifiable causes. This view depersonalizes the universe and makes it more austere, but it also makes it dependable.

(It would here be going too far afield to discuss the principle of indeterminacy or the statistical nature of some other modern physical principles, nor is this necessary for my thesis. Indeterminacy need not deny causality, and statistical prediction is still rationally lawful prediction.)

To those discoveries and principles, which so greatly modified concepts of the cosmos, geology added two more of fundamental, world-changing importance: vast extension of the universe in time, and the idea of constantly lawful progression in time. Estimates of geological time have varied greatly, but even in the 18th century it became clear to a few that the age of the earth must be in millions of years rather than the thousands then popularly accepted from Biblical exegesis. Now some geological dates are firmly established, within narrowing limits, and no competent geologist considers the earth less than 3 billion years old. (Upper estimates for the solar system range from 5 to 10 billion.) That is still only a moment in eternity, but it characterizes a world very different from one conceived as less than 6000 years old.

With dawning realization that the earth is really extremely old, in human terms of age, came the knowledge that it has changed progressively and radically but usually gradually and always in an orderly, a natural, way. The fact of change had not earlier been denied in Western science or theology—after all, the Noachian Deluge was considered a radical change. But the Deluge was believed to have supernatural causes or concomitants that were not operative throughout earth's history. The doctrine of geological uniformitarianism, finally established early in the 19th century, widened the recognized reign of natural law. The earth has changed throughout its history under the action of material forces, only, and of the *same* forces as those now visible to us and still acting on it.

The Higher Superstition and the Discovery of Evolution

The steps that I have so briefly traced reduced the sway of superstition in the conceptual world of human lives. The change was slow, it was unsteady, and it was not accepted by everyone. Even now there are nominally civilized people whose world was created in 4004 B.C. Nevertheless, by early Victorian times the physical world of a literate consensus was geologically ancient and materially lawful in its history and its current operations. Not so, however, the world of life; here the higher (or at least later) superstition was still almost unshaken. Pendulums might swing with mathematical regularity and mountains might rise and fall through millennia, but living things belonged outside the realm of material principles and secular history. If life obeyed any laws, they were supernal and not bound to the physics of inert substance. Beyond its original, divine creation, life's history was trivial. Its kinds were each as created in the beginning, changeless

except for minor and obvious variations.

Perhaps the most crucial element in man's world is his conception of himself. It is here that the higher superstition offers little real advance over the the lower. According to the higher superstition, man is something quite distinct from nature. He stands apart from all other creatures; his kinship is supernatural, not natural. It may, at first sight, seem anomalous that those scientists who held this view did classify man as an animal. Linnaeus, an orthodox upholder of the higher superstition, even classified *Homo* with the apes and monkeys. No blood relationship was implied. The system of nature was the pattern of creation, and it included all created things, without any mutual affinities beyond the separate placing of each in one divine plan.

Another subtler and even more deeply warping concept of the higher superstition was that the world was created for man. Other organisms had no separate purpose in the scheme of creation. Whether noxious or useful, they were to be seriously considered only in their relationship to the supreme creation, the image of God. It required considerable ingenuity to determine why a louse, for example, was created to be a companion for man, but the ingenuity was not lacking. A world made for man is no longer the inherently hostile and evil world of *kanaima,* but that again is offset in some versions of the higher superstition by the belief that man himself is inherently evil or, at least, sinful.

Those elements of the higher superstition dominated European thought before publication of *The Origin of Species,* but various studies of the centennial year have exhaustively demonstrated that evolutionary ideas existed and were slowly spreading among a minority of *cognoscenti* long before Darwin. Some believed that a species, although divinely and separately created, might change, and in particular might degenerate from its form in the original plan of creation. That is not a truly evolutionary view, since it does not really involve the origin of one species from another, but it does deserve to be called proevolutionary in that it recognized the fact that each separate species may change. In the 18th century Buffon went that far, but hardly further, in spite of some apologists who now hail him as an evolutionist.

Some 18th-century worthies—among them Linnaeus in his later years—did go one step further. They conceived that each of the separately created "kinds" of Genesis might later have become considerably diversified, so that the unit of separate creation might be what we now call a genus or even a family or higher group, and the species or subgroups might have arisen, or indeed evolved, since the creation. Just as the many breeds of domesticated dogs are all dogs and of common origin, so the wolves, coyotes, foxes, jackals, and other wild species might all descend from a single creation of the dog-kind. That would still admit no relationship between the dog-kind and the now likewise diversified but singly and separately created cat-kind, for example. (It is an intellectual curiosity that precisely that variation of creationist superstition has recently been seriously revived by an American who had been exposed, at least, to excellent training in zoology.)

By the end of the 18th century there were a few true and thorough-going evolutionists—Charles Darwin's grandfather Erasmus was one, as has so often been pointed out. Their number increased during the first half of the 19th century. Some of them even had glimmerings of Darwin's great discovery, natural selection, although (contrary to some recent historians whose aim seems to be to denigrate Darwin) none of them elucidated that principle clearly and fully.

Darwin

Practically all of the ideas in *The Origin of Species* had been dimly glimpsed, at least, by someone or other before 1859. The only surprising thing about that is that so many centennial authors have thought it worthy of special emphasis. Organization, understanding, and conviction are the main contributions of theorists like Darwin, and obviously none ever succeeded until there already existed something to organize and to understand. It is, however, less obvious why Darwin was the first evolutionist ever to carry conviction to a majority of his fellow scientists. The whole answer is more complex, but its essentials are evident in a statement later made by Thomas Henry Huxley to explain why he was an antievolutionist until he read *The Origin of Species:*

"I took my stand upon two grounds: firstly that up to that time, the evidence in favor of transmutation [evolution] was wholly insufficient; and, secondly, that no suggestion respecting the causes of the transmutation assumed, which had been made, was in any way adequate to explain the phenomena. Looking back at the state of knowledge at that time, I really do not see that any other conclusion was justifiable."

The reason why *The Origin of Species* carried conviction was that it did supply sufficient evidence of evolution and also provided an explanation of the phenomena of evolution. That twofold nature of Darwin's accomplishment has certainly been pointed out often enough, but the statement has also been criticized, and perhaps some small notice should here be given to some of the criticisms. It has, for one thing, been maintained that previous evidence *was* sufficient. It had persuaded Eramus Darwin, Lamarck, Chambers (author of the anonymous *Vestiges of Creation*), and others, so (some critics say) it should have persuaded anyone without Charles Darwin's needing to recompile it. That conclusion is simply ridiculous. What anyone thinks *should* have happened has nothing to do with the question of historical fact. Previous evidence *did not* convince a majority of interested scientists; therefore it was insufficient for that purpose. Darwin's evidence *did* in fact convince them; therefore it was sufficient. (It may of course be recognized, as Darwin himself implied, that the way had been prepared by a changing climate of opinion and that even his evidence might have been insufficient if adduced at an earlier date.)

It has further been suggested that evolution could have been, perhaps should have been, established as a fact without requiring an explanation, and also that Darwin's explanation was not really adequate. The first proposition is debatable, certainly, and examples can be produced to support both sides. The inheritance of acquired characters was accepted by practically everyone, down to and including Darwin, even though no one had adequately explained it. Darwin himself did not like to deal with unexplained facts, and he did belatedly attempt to explain the inheritance of acquired characters. Since in this case the "facts" were not true, that particular Darwinian theory is now charitably forgotten. (Fortunately it was not really essential to his broader theory explanatory of evolution as a whole.) In any case, belief in the inheritance of acquired characters did not

depend on any explanation of the supposed phenomena. (Is there perhaps a warning in the fact that the unexplained phenomena did not in truth occur?) On the other side of the argument is the modern example of extrasensory perception. A great mass of facts is claimed to demonstrate the reality of that unexplained phenomenon, and yet it is not generally accepted. It seems quite clear that it will not carry conviction unless some credible explanation is produced.

It does seem to me highly improbable that the fact of evolution would have been accepted so widely and quickly if it had been unaccompanied by an explanatory theory. Again, to question whether it *should* have been would be childish arguing with history.

The adequacy of Darwin's original explanation of evolution is also decidedly subject to debate. It was certainly an incomplete explanation, as Darwin was keenly aware. We now have much more extensive explanations, built in large part on Darwin's. Parts of Darwin's complex theory are also now known beyond serious doubt to have been wrong, although the more essential parts, those most stressed by Darwin, have been largely substantiated. That is important, and I shall have more to say about it later on. Darwin's theory was adequate at the time in the sense of being convincing. The conviction did not depend entirely on the truth or falsity of different parts of his explanation, which was not wholly accepted by students who nevertheless were immediately persuaded of the truth of evolution. The essential point was demonstration that material causes of evolution are possible and can be investigated scientifically.

The Fact of Evolution

The fact—not theory—that evolution has occurred and the Darwinian theory as to how it has occurred have become so confused in popular opinion that the distinction must be stressed. The distinction is also particularly important for the present subject, because the effects on the world in which we live have been distinct. The greatest impact no doubt has come from the fact of evolution. It must color the whole of our attitude toward life and toward ourselves, and hence our whole perceptual world. That is, however, a single step, essentially taken a hundred years ago and now a matter of simple rational acceptance or superstitious rejection. How evolution occurs is much more intricate, still incompletely known, debated in detail, and the subject of most active investigation at present. Decision here has decidedly practical aspects and also affects our worlds even more intimately, and in even more ways, than the fact of evolution. The two will be separately considered.

The import of the fact of evolution depends on how far evolution extends, and here there are two crucial points: does it extend from the inorganic into the organic, and does it extend from the lower animals to man? In *The Origin of Species* Darwin implies that life did not arise naturally from nonliving matter, for in the very last sentence he wrote, ". . . life . . . having been originally breathed by the Creator into a few forms or into one . . . ." (The words *by the Creator* were inserted in the second edition and are one of many gradual concessions made to critics of that book.) Later, however, Darwin conjectured (he did not consider this scientific) that life will be found to be a "consequence of some general law"—that is, to be a result of natural processes rather than divine intervention. He referred to this at least three times in letters unpublished until after his death, the one from which I have quoted being the last letter he ever wrote (28 March 1882 to G. C. Wallich; Darwin died three weeks later).

Until comparatively recently, many and probably most biologists agreed with Darwin that the problem of the origin of life was not yet amenable to scientific study. Now, however, almost all biologists agree that the problem can be attacked scientifically. The consensus is that life did arise naturally from the nonliving and that even the first living things were not specially created. The conclusion has, indeed, really become inescapable, for the first steps in that process have already been repeated in several laboratories. There is concerted study from geochemical, biochemical, and microbiological approaches. At a recent meeting in Chicago, a highly distinguished international panel of experts was polled. All considered the experimental production of life in the laboratory imminent, and one maintained that this has already been done—his opinion was not based on a disagreement about the facts but on a definition as to just where, in a continuous sequence, life can be said to begin.

At the other end of the story, it was evident to evolutionists from the start that man cannot be an exception. In *The Origin of Species* Darwin deliberately avoided the issue, saying only in closing, "Light will be thrown on the origin of man and his history." Yet his adherents made no secret of the matter and at once embroiled Darwin, with themselves, in arguments about man's origin from monkeys. Twelve years later (in 1871) Darwin published *The Descent of Man,* which makes it clear that he was indeed of that opinion. No evolutionist has since seriously questioned that man did originate by evolution. Some, notably the Wallace who shared with Darwin the discovery of natural selection, have maintained that special principles, not elsewhere operative, were involved in human origins, but that is decidedly a minority opinion about the causes or explanations, not the fact, of evolution.

It is of course also true that the precise ancestry of man is not identified in full detail and so is subject to some disagreement. That is a minor matter of no real importance for man's image of himself. No one doubts that man is a member of the order Primates along with the lemurs, tarsiers, monkeys, and apes. Few doubt that his closest living relatives are the apes. On this subject, by the way, there has been too much pussyfooting. Apologists emphasize that man cannot be a descendant of any living ape—a statement that is obvious to the verge of imbecility—and go on to state or imply that man is not really descended from an ape or monkey at all, but from an earlier common ancestor. In fact, that common ancestor would certainly be called an ape or monkey in popular speech by anyone who saw it. Since the terms *ape* and *monkey* are defined by popular usage, man's ancestors *were* apes or monkeys (or successively both). It is pusillanimous if not dishonest for an informed investigator to say otherwise.

Evolution is, then, a completely general principle of life. (I refer here, and throughout, to organic evolution. Inorganic evolution, as of the stars or the elements, is quite different in process and principle, a part of the same grand history of the universe but not an extension of evolution as here understood.) Evolution is a fully natural process, inherent in the physical properties of the universe, by which life arose in the first place and by which all living things, past or present, have since developed, divergently and progressively.

This world into which Darwin led us is certainly very different from the world of the higher superstition. In the world of Darwin man has no special status other than his definition as a distinct species of animal. He is in the fullest sense a part of nature and not apart from it. He is akin, not figuratively but literally, to every living thing, be it an ameba, a tapeworm, a flea, a seaweed, an oak tree, or a monkey—even though the degrees of relationship are different and we may feel less empathy for forty-second cousins like the tapeworms than for, comparatively speaking, brothers like the monkeys. This is togetherness and brotherhood with a vengeance, beyond the wildest dreams of copy writers or of theologians.

Moreover, since man is one of many millions of species all produced by the same grand process, it is in the highest degree improbable that anything in the world exists specifically for his benefit or ill. It is no more true that fruits, for instance, evolved for the delectation of men than that men evolved for the delectation of tigers. Every species, including our own, evolved for its own sake, so to speak. Different species are intricately interdependent, and also some are more successful than others, but there is no divine favoritism. The rational world is not teleological in the old sense. It certainly has purpose, but the purposes are not imposed from without or anticipatory of the future. They are internal to each species separately, relevant only to its functions and usually only to its present condition. Every species is unique, and it is true that man is unique in new and very special ways. Among these pecularities, parts of the definition of *Homo sapiens*, is the fact that man does have his own purposes that relate to the future—but of man's peculiarities I have more to say below.

Early Naturalistic Theories

The heart of Darwin's explanation of how evolution occurs was natural selection. He always considered this his most important contribution, and posterity agrees with that judgment. It is true that Wallace independently but later reached almost identical views on natural selection and that several others had anticipated both Darwin and Wallace on some points. It is further true that the concept of natural selection has changed through the years since 1859 and that its major importance has occasionally been questioned. Nevertheless, natural selection was primarily Darwin's discovery, later understanding of it has developed from his, and by overwhelming consensus it is now considered the main controlling factor in most evolutionary events.

From the first edition of *The Origin of Species* Darwin expressed the opinion "that natural selection has been the main but not the exclusive means of modification." Yet in the first edition he stressed it almost to the exclusion of other factors. Summing up in the last chapter, he wrote: "Species have changed, and are still slowly changing by the preservation and accumulation of successive slight favorable variations."

That is ambiguous as to what preserves and accumulates the variations, although in context it was obvious that natural selection was supposed to do so. The ambiguity was removed by rewording in the second edition: "Species have been modified, during a long course of descent, by the preservation or the natural selection of many successive slight favorable variations."

There was considerable criticism that Darwin imputed everything, or at any rate too much, to natural selection, and he tended to retreat from so strong a stand. In the fifth edition he changed his previously flat statement by saying that modification of species occurred only "chiefly" through natural selection. In the sixth edition, 1872, the last to be fully revised, Darwin complained that he had been misrepresented, and that he had never thought modification of species due exclusively to natural selection. He made this clear, and unfortunately retreated from a stronger position, by expanding the summary of factors believed to modify species: "This has been effected chiefly through the natural selection of numerous successive, slight, favourable variations; aided in an important manner by the inherited effects of the use and disuse of parts; and in an unimportant manner, that is in relation to adaptive structures, whether past or present, by the direct action of external conditions, and by variations which seem to us in our ignorance to arise spontaneously."

That summarizes the full and final Darwinian theory, which thus recognizes four factors or causes of evolution, in sequence of importance in Darwin's opinion: (i) natural selection; (ii) inheritance of acquired characters due to use or disuse of organs; (iii) inheritance of acquired characters due to direct effects of the environment; (iv) what we now call mutations in the broadest sense.

Darwin rejected, without even mentioning them, various dualistic, vitalistic, or otherwise nonmaterialistic theories of evolution already proposed by 1872. He accepted only factors that were believed to be strictly materialistic or naturalistic, but among those he played safe. He accepted them all, although he considered the last two unimportant as explanations of adaptation. Later in the 19th century there was an interesting parceling out of Darwin's four factors into three distinct theories, each emphasizing one or two of those factors at the expense of the others.

One school took the attitude of which Darwin had, as he felt, been falsely accused. They emphasized Darwin's first factor, natural selection, and flatly rejected almost any others, explicitly the inheritance of acquired characters, whether acquired from habit or from environmental influence. Their theory, more Darwinian than Darwin's, came to be called flatly Darwinism or, more specifically, Neo-Darwinism.

A second school of theory accepted and emphasized the inheritance of acquired characters, Darwin's second and third factors, and minimized without necessarily wholly rejecting the influence of natural selection and of mutation. That theory is now usually called Lamarckian or Neo-Lamarckian, but the designations are misleading. I shall not here take the time to discuss Lamarck's own theory, which never gained any important adherents in its original form. Neo-Lamarckism, which has more strongly influenced evolutionary studies, rejects the very heart and basis of Lamarck's personal theory, which was an idealistic and vitalistic view of continuous and continual climbing of a "ladder of nature," from simple to complex beings. Neo-Lamarckism also stresses a factor that Lamarck rejected: inheritance of direct effects of the environment. Neo-Lamarckism is more Darwinian than Lamarckian and is, indeed, about as Darwinian as Neo-Darwinism. It emphasizes Darwin's second and third factors rather than his first one, but it does not wholly reject any Darwinian factor, and it includes nothing that was not explicitly accepted by Darwin.

The third theory here in question emphasized Darwin's fourth factor, his

"variations which seem to us in our ignorance to arise spontaneously," now called mutations. This was not, however, a development of parts of Darwinian and to some extent pre-Darwinian theory, as both Neo-Darwinism and Neo-Lamarckism were. To Darwin, mutation (not yet under that name) was only one way, and the least important way, in which materials for evolution arose. The mutationists were striking out along quite new lines, developing modern genetics and rediscovering Mendelism. The extreme mutationists, notably De Vries, held that mutations were the *only* way in which significant evolutionary change occurs. They reduced natural selection to the minor and negative role of eliminating mutants so grossly malformed as to be inviable. They agreed with the Neo-Darwinians in denying the reality of the inheritance of acquired characters.

Those three theories, tagged as Neo-Darwinism, Neo-Lamarckism, and mutationism, seemed in the early 20th century to be the principal if not the only alternatives as naturalistic explanations of evolution. They have in common the fact that they are naturalistic. That is, they hold that evolution is a consequence of the material, physical properties of the universe and that it is explicable without postulating any immediate nonphysical, non-natural influences. Beyond that quite basic philosophical point, the three theories do lead to three different views of the world and of man's nature and potentialities.

In the Neo-Darwinian view, the crucial point in evolutionary change is the comparative success of genetical variants in producing offspring. Given a store of varying genetical materials within a population, natural selection usually tends to produce and to increase genetical combinations that are likely to ensure survival and continued reproductive success for the group as a whole. Genetical variation in itself is not considered adaptive in origin, and it is not *directly* influenced by any needs, desires, or activities of individuals in the population. Yet genetical change through the generations is decidedly nonrandom, as a rule, and tends to be adaptive for the population. To that extent, the Neo-Darwinian theory is still accepted by a majority of biologists today. It has not been rejected but only modified by being integrated into a synthesis that is both broader and deeper. The Neo-Darwinian world view originally stressed individual survival, especially competitive success. The later synthesis has involved considerable modification of that emphasis.

The Neo-Lamarckians give prime importance to exactly those factors that were minimized by the Neo-Darwinians: the needs, desires, and activities of individuals. Those factors, together with the modifying influences of soil, climate, food, and other environmental features, are supposed to lead directly to evolutionary change. Thus, genetical modification is supposed to be adaptive in its very essence. The entire process is oriented by the direct, unmediated reactions of individual organisms to their environments. The simplicity of this view is appealing, and it also has a special emotional attraction. It suggests that personal accomplishment counts not only in one's own lifetime but also in posterity and in the eventual evolution of the human species. Improvement in physique by exercise, diet, and so on, may lead to stronger descendants, and education may lead to more intelligent ones. A world in which that was true would on the whole be a pleasant one, and also one in which human progress would be comparatively easy to control. Undoubtedly it is that appeal and its political implications that have made a form of Neo-Lamarckism popular among the rulers of the Soviet Union. As I have already pointed out, there is justification for not labeling this theory with the name of Lamarck. The Russians variously call it "Soviet creative Darwinism" (as opposed to capitalistic and unacceptable Neo-Darwinism), "Michurinism," or "Lysenkoism." Although it is improbable that any of the really able Russian biologists fully accept that theory in private, it is publicly approved Communist dogma.

The only trouble with Neo-Lamarckism in any of its various seductive guises is that it is not true. Now that we understand the mechanism of inheritance, which Darwin could not know, it is certain that acquired characters cannot possibly be inherited in the way demanded by this theory, and that is that.

The extreme mutationist world view is very different from either the Neo-Darwinian or the Neo-Lamarckian. In it evolution is dominated by chance. Change within species or from one species to another is believed to be not only initiated but also carried through by a mutation or, eventually, a sequence of mutations. Mutations certainly have definite physical causes, even though these are unknown in most specific instances, and they have determinate effects. They arise, however, by chance, and their effects are random in the sense that the cause of a mutation has no evident relationship to the nature of the result and that the effects are unoriented with respect to usefulness or adaptation in the organism. The same cause, such as radiation, may result in any and all kinds of mutations, none producing changes adaptively related to the original radiation. Furthermore, if, for instance, animals are in a situation where increase in size would be adaptive, mutations for larger size do not thereby become either more or less frequent. Mutants are in these senses random or accidental. If there just happens to be some niche into which they can fit, they survive, and a step in evolution has occurred. A recent form of the theory calls such lucky mutants "hopeful monsters." If the mutant does not happen to fit anywhere, it dies, and that is all. Evolution in the mutationist world is not merely aimless but also directionless.

That mutations occur and are random in the stated sense of that word are facts established by innumerable observations. Mutationism, unlike Neo-Lamarckism, rests on a basis of real phenomena. Nevertheless, the further deductions drawn by the original and the extreme mutationists are flatly contradicted by other phenomena, notably those of adaptation. The origin of such an organ as an eye, for example, entirely at random seems almost infinitely improbable. Added to such considerations are many paleontological examples showing evolution occurring through millions of years not fitfully and haphazardly but in a perfectly definite and manifestly adaptive way. The theory that the direction of evolution is fully controlled by mutation simply cannot be true.

Synthesis

Adaptation and the apparent purposefulness of evolution are basic problems that a successful theory *must* solve. The rising science of genetics early in this century not only failed to solve the problem but also made it appear insuperably difficult. That explains why almost no students of other disciples were inclined to accept mutationism,

and why Neo-Lamarckism, an elegant but as we now know incorrect solution, hung on for so long. It also was one of several reasons for continued popularity of non-naturalistic theories, to which I allude below.

The way out of the dilemma seems simple now that it has been found. Mutationism is not an alternative to Neo-Darwinism but a supplement to it. If mutation is the source of new variation and yet is substantially nonadaptive, and if the actual course of evolution is to a large extent adaptive, then some additional factor or process must frequently intervene between the occurrence of mutations and the incorporation of some of them into evolving populations. The intervening process must be literally selective, because it must tend (not necessarily with full efficiency) to weed out disadvantageous mutations and genetic combinations and to multiply those that are advantageous in existing circumstances. Natural selection is just such a process, and the principal modern theory of evolution, although it contains much besides, is in large part a synthesis of selection theory and mutation theory.

Evolution is an extremely complex process, and we are here interested mainly in the effects of the concept on our world rather than in the process for its own sake. For that purpose I must, however, briefly note the main elements of the process now known. Genetic systems, governing heredity in each individual case, are composed of genes and chromosomes, discrete but complexly interacting units at different levels of size and complexity. The genes themselves, their organized associations in chromosomes, and whole sets of chromosomes have a large degree of stability as units, but all the kinds of units are shuffled and combined in various ways by the sexual processes of reproduction in most organisms. Thus, a considerable amount of variation is maintained, and, so to speak, genetic experimentation occurs in all natural populations. Mutations, in the broadest sense, affecting individual genes, chromosomes, or sets of chromosomes, introduce wholly new variation, which is fed into the processes of recombination.

Populations of similar animals, usually interbreeding among themselves and definable as species, have genetic pools, characterized by the total of genetic units in the included individuals and the distribution of combinations of those units through the population. Evolutionary change involves changes in the genetic pool, in kinds of included units, in frequencies of them, and in kinds and frequencies of combinations of them. Recombination alone does not tend to change the genetic pool. Only three processes are known to do so: mutation, fluctuation in genetic frequencies (what are known statistically as "sampling errors"), and differential reproduction. The first two of those processes are not oriented toward adaptation. They are in that sense essentially random, and are usually inadaptive, although they may rarely and coincidentally be adaptive. By "differential reproduction" is meant the consistent production of more offspring, on an average, by individuals with certain genetic characteristics than by those without those particular characteristics. That is the modern understanding of natural selection, including but broader than the Darwinian or Neo-Darwinian concept, which emphasized mortality and survival more than reproduction. Natural selection in the Darwinian sense and still more in this expanded sense is nonrandom, and its trend is adaptive. It also tends, not always with complete success, to counteract the random effects of mutation and sampling error.

Evolutionary processes are tremendously more complicated in detail than this bald outline suggests. The point of the outline is that here is a mechanism, involving only materials and processes known beyond a doubt to occur in nature, capable (as one of its proponents has said) of generating just the degree of improbability evident in the phenomena of evolution.

Further information pertinent to our theme is provided by paleontology, the actual record of events in the history of life. Observation and experimentation with living organisms can extend over a few years, at most. There is always a possibility that processes there evident worked out differently over spans of millions of years, or that the actual history involved principles undetectable in shorter periods of time. There is admittedly some difference of opinion, but I think it fair to say that there is now a consensus for the view that the fossil record is fully consistent with the modern synthetic theory of evolution and that it neither requires nor suggests any alternative explanation.

There is one thing demonstrated by the fossil record that is decidedly pertinent here and that probably would never have been inferred from study of living organisms. Throughout the whole history of life most species have become extinct, without issue. The statistically usual outcome of evolution is not, then, the progressive appearance of higher forms but simply obliteration. There has, indeed, been progression and even (still more rarely) progress, but this has been in the comparatively few, exceptional lines of descent. The adaptive mechanism of natural selection has guaranteed that some lineages would win, that the world would indeed be filled and kept filled with adapted organisms, but just as inexorably it has insured that most lineages would lose. It has, moreover, had the result that even the winners, the lineages that have survived so far, have not necessarily been progressive, from a human point of view at least. The primitive ameba has remained adapted, hence has survived, while the lordly dinosaurs lost adaptation and therefore life. The degenerate tapeworm is to all appearances as well adapted as the—we like to think—progressive man.

Naturalism, Vitalism, Finalism

The theory just outlined obviously does not yet answer all questions or plumb all mysteries, even when the details here omitted are taken into consideration. It casts no light on the ultimate mystery—the origin of the universe and the source of the laws or physical properties of matter, energy, space, and time. Nevertheless, once those properties are given, the theory demonstrates that the whole evolution of life could well have ensued, and probably did ensue, automatically, as a natural consequence of the immanent laws and successive configurations of the material cosmos. There is no need, at least, to postulate any non-natural or metaphysical intervention in the course of evolution.

That conclusion has been questioned or opposed not only by many philosophers and theologians but also by a comparatively small number of scientists. The alternatives occasionally supported by scientists or scientific philosophers, and therefore pertinent here, comprise many shadings and variations of opinion, but most of them can be placed in the rubrics of vitalism and finalism.

The vitalists maintain that life is an

essence or principle in itself, absent in nonliving matter and not reducible to the interaction of fully material factors. They usually point to a directedness or apparent purposefulness in the development and activities of living things and conclude that the vital, nonmaterial essence within them is a controlling influence in evolution. The finalists maintain that the evolutionary history of life has a preordained over-all pattern which, at least until the appearance of man, was purposefully directed toward a future goal or end. There is no absolute logical necessity that vitalism and finalism should go together, but the ideas are related if only because both are to some degree non-naturalistic and, in that sense, nonmaterialistic. More often than not, vitalists are finalists and finalists are vitalists.

Darwin's legacy in this respect was somewhat but not altogether negative. He did not discuss these issues explicitly and in plain terms. From the whole body of his work, and perhaps more particularly from notes and letters not written for publication, it is clear enough that he felt an antipathy for these philosophical approaches. The very fact that he did not specifically go into these problems amounts to a tacit but positive stand that metaphysical postulates are not necessary for a scientific explanation of evolution.

To that extent it is quite true, as has been so often said, especially by his enemies, that Darwin was a materialist. *Materialist* has become a highly ambiguous word and in some circles a dirty one. It is better here to use the word *naturalist*, in the proper philosophical sense of a scientific inquirer who eschews recourse to the supernatural. Such an inquirer does not deny the possible existence of the supernatural but only excludes it from attempts at scientific explanation. Almost all scientists agree that such exclusion is pragmatically justified and indeed necessary. Appeal to the unknown or to the scientifically untestable always stultifies the progress of science, because it stops the search for material explanations that *are* scientifically testable—and that, as a matter of experience, have generally been forthcoming when the search has been continued.

Most scientific evolutionists since Darwin have followed his lead in this matter and have continued to seek material, natural explanations of evolution without necessarily taking any overt stand on vitalism or finalism. To the extent that vitalism and finalism are nontestable, that attitude is justified, and the scientist, as scientist, has no right to go further than to repeat the classic remark that he has no need of that hypothesis. However, I do not see how the matter can in all candor be dropped at that point even by the least philosophical of evolutionists, for there are repeated claims by vitalists and finalists that their views *are* testable and that there *is* need for that hypothesis.

In the space available I cannot discuss concrete items of evidence but can only rather flatly state conclusions. These conclusions are not accepted by all evolutionists, but I think it safe to say that they are by most. The sort of testable evidence that would suggest vitalism or finalism would be the steady progression of life, and of each of its evolving lineages, toward a final and transcendentally worthy goal. That is not, in fact, what the known record of life's history shows. There is no clear over-all progression. Organisms diversify into literally millions of species, then the vast majority of those species perish and other millions take their places for an eon until they, too, are replaced. If that is a foreordained plan, it is an oddly ineffective one. Single lineages, when they can be followed for long, often do show rather steady change, but not indefinitely. They become extinct, or if they survive, the directions and rates of their evolution change. They evolve exactly as if they were adapting as best they could to a changing world, and not at all as if they were moving toward a set goal. As for the directedness that does indeed characterize vital process, it is amply explicable by natural selection without requiring any less mundane cause.

That sort of evidence, with much else in detail, convinces me, at least, that the hypotheses of vitalism and finalism are not necessary. Everything proceeds as if they were nonexistent. That does not prove that they are untrue, but it makes their positive adoption unjustified.

Vitalism and finalism have one other aspect that has no particular scientific bearing but that does require mention. They are sometimes advanced with the avowed hope of retaining something from the world of superstitution. Vitalism then pretends to find a place in nature for the supernatural. Finalism tries to bring in by the back door the teleology that Darwin swept out the front door.

The World of Man

Let me summarize and conclude as to this world into which Darwin led us. In it man and all other living things have evolved, ultimately from the nonliving, in accordance with entirely natural, material processes. In part that evolution has been random in the sense of lacking adaptive orientation. As a rule, however, it has been oriented or directed toward achieving and maintaining adaptive relationships between populations of organisms and their whole environments. Nevertheless, this blind, amoral process has not guaranteed indefinite maintenance of adaptation for any given lineage of populations. On the contrary, it usually leads to eventual extinction and a repeopling of the world by the newly divergent offspring of a minority of earlier successful lineages. The mechanism of orientation, the nonrandom element in this extraordinarily complex history, has been natural selection, which is now understood as differential reproduction.

Man is one of the millions of results of this material process. He is another species of animal, but not just another animal. He is unique in peculiar and extraordinarily significant ways. He is probably the most self-conscious of organisms, and quite surely the only one that is aware of his own origins, of his own biological nature. He has developed symbolization to a unique degree and is the only organism with true language. This makes him also the only animal who can store knowledge beyond individual capacity and pass it on beyond individual memory. He is by far the most adaptable of all organisms because he has developed culture as a biological adaptation. Now his culture evolves not distinct from and not in replacement of but in addition to biological evolution, which also continues.

Concomitant with these developments is the fact that man has unique moral qualities. The evolutionary process is not moral—the word is simply irrelevant in that connection—but it has finally produced a moral animal. Conspicuous among his moral attributes is a sense of responsibility, which is probably felt in some way and to some degree by every normal human being. There has been disagreement and indeed confusion through the ages regarding to whom and for what man is responsible. The lower and the higher superstitions have produced their several an-

swers. In the post-Darwinian world another answer seems fairly clear: man is responsible to himself and for himself. "Himself" here means the whole human species, not only the individual and certainly not just those of a certain color of hair or cast of features.

The fact that man knows that he evolves entails the possibility that he can do something to influence his own biological destiny. The fact that uncontrolled evolution often leads to degeneration and usually to extinction makes it highly advisable that man take a hand in determining his own future evolution. If man proceeds on the wrong evolutionary assumptions—for instance, on those of Neo-Lamarckism or Michurinism—whatever he does is sure to be wrong. If he proceeds on the right assumptions, what he does may still be wrong, but at least it has a chance of being right.

A world in which man must rely on himself, in which he is not the darling of the gods but only another, albeit extraordinary, aspect of nature, is by no means congenial to the immature or the wishful thinkers. That is plainly a major reason why even now, a hundred years after *The Origin of Species*, most people have not really entered the world into which Darwin led—alas!—only a minority of us. Life may conceivably be happier for some people in the older worlds of superstition. It is possible that some children are made happy by a belief in Santa Claus, but adults should prefer to live in a world of reality and reason.

Perhaps I should end on that note of mere preference, but it is impossible to do so. It is a characteristic of this world to which Darwin opened the door that unless *most* of us do enter it and live maturely and rationally in it, the future of mankind is dim, indeed—if there is any future.

Science in the News

Environmental Radiation Studies Begun by Public Health Service in New Mexico and Missouri

Two long-range studies of the effects of environmental radiation on the health of large populations were begun in March by the U.S. Public Health Service in cooperation with state and local health agencies. One study is in San Juan County, New Mexico, site of one of the largest uranium-producing areas of the country. Earlier studies showed the radioactivity from radium in the surface water of the Animas River in San Juan County to be higher than the level in most areas in the United States. The other study is in the St. Louis, Missouri, region. Earlier studies showed levels of strontium-90 to be somewhat higher in the St. Louis milkshed than in other areas.

The San Juan project, on which preliminary work has already begun, involves detailed medical and laboratory examinations of approximately 100 families totaling about 400 individuals. Teams of federal and state physicians, nurses, and technicians will obtain complete medical histories of each individual in the cooperating families and will determine a typical week's diet. The typical diets will be analyzed to determine the amount of radioactivity taken in. Body wastes and breath samples will be collected and analyzed to determine the amount of radioactivity excreted.

Exhaustive study will also be made of vital statistics for the area. Some aspects of the research project will require follow-up interviews, medical examinations, laboratory studies, and statistical analyses over a period of several years.

Arrangements have been completed to develop laboratory facilities and offices for the staff of the project in the San Juan District Health Department Building in Farmington, N.M. Laboratory analyses will be performed there and at the new Public Health Service Laboratory in Las Vegas, Nev.

Howard McMartin of the Public Health Service will be the medical officer in charge of the field activities. He will be assisted by six full-time resident staff members and four part-time staff members. The San Juan County Health Department and the County Medical Society will cooperate in the project.

Describing the new study, Surgeon General Leroy E. Burney of the Public Health Service said that selection of San Juan County for the first of these radiation studies does not mean that the health of people in this area is known to have been adversely affected by environmental radiation. On the contrary, official health records and observations of local physicians indicate no unusual health problems.

Effective steps have already been taken to reduce the amounts of radioactive waste discharged into the rivers in this area from milling operations, Burney said. However, the extensive data recently obtained on environmental radioactivity in the area presents a good opportunity to determine through further study the amounts of radioactivity that are currently being taken in by people, the amounts retained, the total body burden, and the effects upon their health.

Plans for the St. Louis Project

The St. Louis project will begin with a preliminary survey of dairy farms in the St. Louis milkshed. The survey will consist of investigations of water supplies, sources of animal food, climate, farming practices, animal feeding practices, and other variables that may be associated with different types and levels of radioactivity in milk. The final phase of the milkshed study will consist of field experiments to determine whether, if necessary, the level of radioactivity in milk can be reduced by modifications in dairy-farm practices.

The St. Louis study is an outgrowth of negotiations over the past several months that culminated in agreements among the federal, county, and city health agencies. Under these agreements the federal government will reimburse the St. Louis County Health Department for the cost of personnel, mate-

rials, supplies, and travel directly involved in the first phase of the study. The County Health Department is authorized to enter into agreements with local health departments in adjacent states concerning the collection and delivery of samples of milk, water, cattle feed, and other materials to be analyzed. The federal government will furnish certain necessary equipment to the St. Louis group, and the Public Health Service's Sanitary Engineering Center in Cincinnati, Ohio, will assist in laboratory analysis of samples.

The federal government's share of the costs for the remainder of fiscal year 1960 will amount to $35,700; its share for fiscal year 1961 will be about $100,000.

Because of the quantity of general information to be obtained in environmental radiation studies, it is expected that more elaborate investigations will follow the studies in San Juan County and the St. Louis area. Such additional research would be part of the nationwide effort of the Public Health Service and of state health departments to determine the significance of radioactivity in the environment.

Eisenhower and Macmillan Hold Talks on Soviet Call for a Moratorium on Underground Tests

On 19 March the Soviet delegate to the three-nation talks on a nuclear test ban called a special meeting—the 188th since the Geneva talks began. Semyon K. Tsarapkin announced Soviet willingness to accept, with one condition, President Eisenhower's proposal of 11 February, which called for the United States, Great Britain, and the Soviet Union to agree on a controlled ban of high-powered blasts. The condition was that the three nations must also agree on an unpoliced moratorium on small underground explosions.

Tsarapkin explained that during the moratorium there would be further research on methods of detecting smaller explosions, but he did not fix the duration of the moratorium and he did not discuss what would happen at its conclusion. Observers in this country expect that the Russians have in mind a moratorium of 4 or 5 years, possibly longer.

The Soviets have been insisting, until their most recent proposal, that a treaty must ban all nuclear explosions: in the atmosphere, in outer space, under ground, and under water. President Eisenhower proposed that underground blasts of less than 19 kilotons be excluded from the treaty until measures for policing them could be agreed upon.

Initial Reaction

The Soviet suggestion has been the subject of lively debate in Washington. According to reports, Secretary of State Christian A. Herter, Secretary of Defense Thomas S. Gates, Jr., John A. McCone, chairman of the Atomic Energy Commission, Allen W. Dulles, director of the Central Intelligence Agency, and George B. Kistiakowsky, the President's scientific adviser, found it impossible at a meeting on 22 March to reach an agreement on the United States response. The participants agreed that the Soviet proposal was not acceptable as it now stood, but Herter suggested a counteroffer, while Gates and McCone favored standing by the earlier American proposal for a limited ban.

Also on 22 March, the chairman of the Joint Congressional Atomic Energy Committee, Senator Clinton P. Anderson, Democrat of New Mexico, called the Soviet offer "phony." He claimed that the proposed moratorium on small underground explosions, if accepted, would achieve the Soviet goal of a total ban without proper controls. Anderson's remarks are significant because any test ban treaty must eventually go to the Senate, and the joint atomic energy committee would play an important part in the debate on ratification.

As the week progressed, however, the Administration seemed to be moving towards some kind of counteroffer. At his news conference on 25 March, Secretary of State Herter said that the Soviet plan was neither "completely unacceptable" nor "completely acceptable." He indicated that the President might view favorably a "relatively brief" testing moratorium on small underground blasts. Further, Herter pointed out that the latest Soviet proposal recognized for the first time Western reservations about the efficacy of present detection methods in distinguishing between small underground explosions and earthquakes. On 26 March Senator Anderson said that perhaps the United States should chance a 1-year moratorium on the smaller underground tests.

Eisenhower and Macmillan Meet

On the same day that Anderson gave his new appraisal Prime Minister Harold Macmillan of Great Britain and his staff flew to Washington to confer with President Eisenhower about the Western reply to the Soviet offer. The general impression is that the British are willing to assume more risks than the United States to get a test ban treaty. They feel that the Soviet offer represents a step that is worth exploring. Their fear, at least at the beginning of the week when Macmillan's trip was first announced, was that the United States favored a flat rejection of the new Soviet proposal.

The small party Macmillan brought with him from London included Con D. W. O'Neill, Foreign Office Under Secretary in charge of disarmament; Sir Norman Brook, Secretary to the Cabinet; and Sir William Penney, a member of the British Atomic Energy Authority.

The Prime Minister met with Herter on the morning of 28 March for preliminary discussions, and in the afternoon went with President Eisenhower to Camp David, the presidential retreat in the Catoctin Mountains of Maryland, 65 miles from Washington. Vice President Nixon and Secretary Herter joined in the talks with Macmillan.

A number of other points besides the question of a moratorium must be nailed down before the United States, Britain, and the Soviet Union can sign a treaty. The question of the nationality of the head of the commission responsible for policing tests must be settled, as must the composition of the staff at the control posts. And there is still the matter of how many on-site inspections are to be permitted.

Food Additives Law Reported To Be Curtailing Research

Research on additives for animal feeds may be the first casualty of the 1958 Food Additives Amendment, says the 21 March issue of *Chemical and Engineering News*. (The amendment to the Federal Food, Drug and Cosmetic Act was passed in 1958 but became fully effective only in March.) Drug and chemical manufacturers surveyed by the American Chemical Society weekly say that application of the amendment to drugs for use in feeds

is extremely difficult and cumbersome.

One producer, Chas. Pfizer and Co., has substantially cut its budget for agricultural research and development and has transferred scientific personnel from its agricultural research center to other divisions. The scientists will be returned to agricultural research if legislative revision or changes in interpretation reduce the difficulties now faced by the company in getting its products cleared for veterinary use.

Another pharmaceutical firm, Eli Lilly and Co., is shifting its agricultural research efforts from work on hormones and thyroxin blockers for use as feed additives to pesticides for household, farm, and industrial use. The company has found that in some instances, and in some animals, these compounds have caused hyperplasia—an increase in cell growth which is sometimes a forerunner of cancer. A compound that causes hyperplasia may come under the amendment's Delaney Clause, which prohibits the use of any amount of any cancer-causing agent in any animal or human food.

Companies that make direct food additives such as emulsifiers, antioxidants, and preservatives have been less affected by the new amendment. Such companies, reports *Chemical and Engineering News*, are continuing their research on the present basis—in some cases even expanding it. For direct additives, the magazine reports, the amendment's effect is merely to formalize the testing procedures the companies had been following all along.

Many chemical products never looked upon as food additives—for example, paper, plastics, and petroleum-based materials—must now be considered in an entirely new light. These products go into the making of cartons, containers, and packages used by the food industry. Rubber items such as conveyors, hose, gaskets, seals, and parts for milking machines also must be re-evaluated.

Effects of the amendment in other areas are not yet clear-cut, the magazine reports. Pesticides, for example, have become suspect in the public mind, even though pesticides do not come under the Food Additives Amendment. So far, the magazine finds, the search for new pesticides and for new uses for older ones continues unabated. But pesticide manufacturers are joining with producers of food additives in criticizing the Delaney Clause of the new amendment.

Office of Education Studies University Finances

The approximately 2000 colleges and universities in the United States are receiving increased financial support, are spending more on educational activities, and are extending their holdings, the U.S. Office of Education announced in releasing advance totals from its biennial survey of the finances of higher education institutions. The survey covers the year 1957–58. Expenditures by colleges and universities for day-to-day activities rose 29 percent, from $3.5 billion in 1955–56 to $4.5 billion in 1957–58.

Total expenditures for additions to plant totaled $686 million in 1955–56 and $1.1 billion in 1957–58. To reach these levels of expenditure for plant, institutions transferred $130 million in 1955–56 and $171 million in 1957–58 from current operating funds.

Altogether, more than $3.6 billion was spent on educational activities at the 1940 institutions included in the survey. This figure is about 30 percent above the 1955–56 level.

Government Supports Research

The institutions expended $734 million for organized research during 1957–58, 45 percent above the level of 1955–56. However, almost 75 percent of the $734 million was paid by the federal government.

Other items of expense reported in the study were fellowships, scholarships, other forms of student aid, and such expenditures as maintenance of dormitories and student dining halls.

While expenditures increased, the value of plant and endowment rose 25 percent, from $12.7 billion to just under $16 billion. Buildings, grounds, and equipment of the institutions were valued at more than $11 billion in 1958, as compared with something over $8.9 billion 2 years before. Endowment funds totaled $4.6 billion in 1958 and something over $3.7 billion in 1956.

During these years the increase in student enrollment was nearly 15 percent and the per capita income of the nation rose nearly 10 percent. The colleges and universities obtained almost one-third of their income for educational purposes from state appropriations. Another one-fourth was obtained by tuition payments from students. The remainder came from the federal government, from private gifts, from local governments, and from other, miscellaneous sources. These figures are the totals for both publicly and privately controlled institutions.

Processing of the data obtained in the study has not yet reached a point where analysis by type of control (public or private) or by location of institution is practicable.

Mathematics Study Group Announces New Paperback Series

A new paperback series entitled *Student's Mathematical Library*—designed to present mathematics as "a meaningful human activity" to students and readers in general—will begin publication early in 1961. Published jointly by Random House and Wesleyan University, the books will sell for less than $1 each.

Headquarters for the *Student's Mathematical Library* will be New York University's Institute of Mathematical Sciences. Lipman Bers, chairman of the department of mathematics at NYU's Graduate School of Arts and Science, is chairman of the Library's editorial panel, and Anneli Lax, assistant research scientist at the institute, is the Library's technical editor.

Many of America's leading mathematicians and scientists will be contributors. The first books to appear will be concerned with irrational numbers, infinity, mathematical logic, logarithms, and calculus.

Through the series, readers with no more than a rudimentary knowledge of mathematics will have the opportunity to advance to subjects previously presented only at advanced levels.

The new series is an activity of the School Mathematics Study Group, a national association whose goal is to improve the study of mathematics in American schools and to introduce to capable students and interested laymen aspects of mathematics that most of today's courses do not treat. Formed with the financial aid of the National Science Foundation, the Study Group has its headquarters at Yale University.

Grants, Fellowships, and Awards

Essay prizes. Prizes totaling 100 guineas are offered by England's Imperial Chemical Industries Limited, publishers of the quarterly scientific review *Endeavour*, for essays on scientific subjects. In addition to the cash prizes the

prizewinners will receive invitations to attend the whole of the 1960 meeting of the British Association for the Advancement of Science, to be held in Cardiff from 31 August to 6 September. All expenses will be paid, including traveling expenses within the United Kingdom.

As one of the primary purposes of these awards is to stimulate younger scientists to take an interest in the work of the British Association, the competition is restricted to those whose 25th birthday falls on or after 1 June. Five prizes will be awarded: a first prize of 50 guineas; a second prize of 25 guineas; a third prize of 15 guineas; and two special prizes of 5 guineas each for competitors who have not passed their 18th birthday on 1 June.

The subjects for the essays are "The Structure of the Atomic Nucleus," "Science and Agriculture," "The Moon," "The Role of Chance in Scientific Discovery," "Modern Methods of Chromatography," and "The Ocean Depths." The essays, which must be in English and typewritten, should not exceed 4000 words in length; only one entry is permitted from each competitor. All entries should be addressed to: The Deputy Secretary, British Association for the Advancement of Science, 18 Adam St., Adelphi, London, W.C.2, and the envelope should be clearly marked "Endeavour Prize Essay." The latest date for receipt of entries is *1 June*.

The essays must be submitted without signature. The competitor's full name and address and date of birth should be given in a sealed covering letter attached to the essay and addressed to the Deputy Secretary of the British Association, who will acknowledge all entries. Special attention will be paid to originality of approach and to syntax and literary style. The competitor's age will also be taken into account. The essay winning the first prize will be published in *The Advancement of Science*, journal of the British Association.

Heart reporting. The American Heart Association (44 E. 23rd St., New York 10) has announced that *1 May* is the deadline for entries in the annual competition for its Howard W. Blakeslee Awards, made in recognition of outstanding reporting in the field of heart and blood-vessel diseases. Newspaper and magazine articles, books, radio and television programs, and films published or produced between 1 March 1959 and 29 February 1960 are eligible for the competition.

The Blakeslee Awards, which carry an honorarium of $500 each, will be presented in the fall. The number of winners to be selected will be determined by the AHA Awards Committee. The awards are given to "individuals whose creative efforts in any local or national medium of mass communication are judged to have contributed most to public understanding of progress in research, and in the prevention, care, and treatment of heart and circulatory diseases."

News Briefs

Laboratory animal diseases. The seventh annual course on the pathology of diseases of laboratory animals, presented by the Armed Forces Institute of Pathology, Washington, D.C., is scheduled for 26–30 September. The course is designed to provide training for scientists who are responsible for the recognition and interpretation of lesions in experimental animals, or who have charge of procurement and maintenance of animal colonies. Although this course is primarily designed for military personnel, a limited number of civilian scientists may attend. Application forms may be obtained by writing to the Director, Armed Forces Institute of Pathology, Washington 25, D.C. Applications should be submitted before 15 August.

* * *

High-school biology. Sample units of the American Institute of Biological Sciences' new motion-picture biology course for high schools will be shown on request at meetings of teachers, school administrators, state or regional high school parent-teacher associations, and other groups. H. Burr Roney, project director for the course, reports that biological scientists will bring the films to such meetings and will speak about the content and use of the course. There will be no charge to sponsoring organizations. Program chairmen may obtain speakers and showings of the films by writing to Jack Steuerwald, AIBS Film Series, 200 P St., NW, Washington 6, D.C.

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Crustacea. The first issue of a new quarterly journal, *Crustaceana*, was published in January by E. J. Brill, of Leiden, The Netherlands. The objective of the new international journal is rapid publication of papers on crustacean research in all its aspects, including taxonomy, ecology, physiology, anatomy, genetics, paleontology, and biometry. Information may be obtained in this country from editorial board member Professor Arthur G. Humes, Department of Biology, Boston University, Boston 15, Mass.

* * *

Pacific science. The 10th Pacific Science Congress of the Pacific Science Association will be held at the University of Hawaii, Honolulu, from 21 August to 6 September 1961, under the sponsorship of the U.S. National Academy of Sciences and the Bernice P. Bishop Museum, with the cooperation of the University of Hawaii. Scientific sessions will be held from 21 August to 2 September, to be followed by a field trip that will continue through 6 September.

This congress will include a Section of Science Information, with Burton W. Adkinson as section organizer. Adkinson is head of the Office of Science Information Service, National Science Foundation, Washington 25, D.C. Science information will be discussed in terms of improving scientific communication and documentation practices between countries in the Pacific Ocean area.

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Russian biology review. The Institute of Biology, 41 Queen's Gate, London S.W.7, is collaborating with the Department of Scientific and Industrial Research and with Oliver & Boyd, Ltd., in the production of an English translation of *Russian Review of Biology*. The first issue of the translation has been released.

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Neuropathology. The 4th International Congress of Neuropathology will be held in Munich, Germany, 3–8 September 1961. The main themes are histochemistry and related biochemistry of diseases of the central and peripheral nervous systems, electron microscopy of the central and peripheral nervous systems and of the myoneural junction, and cell biology and cell culture of nervous tissue. Deadline for abstracts of papers on these themes is 1 June 1960. There will also be papers on perinatal neuropathology and free papers. Those wishing to attend should notify the chairman of the American committee: Dr. Orville T. Bailey, Neuropsychiatric Institute, 912 S. Wood St., Chicago 12, Ill.

Science in the News

Three scientists from the United Kingdom will be in the United States to attend the conference on mammalian genetics and reproduction at Gatlinburg, Tenn., 4–7 April.

B. M. Cattanach, a member of the Medical Research Council's scientific staff at the Mutagenesis Research Group, Edinburgh, will visit Washington, New York, and Oak Ridge (Tenn.) before returning on 10 April.

Mary F. Lyon, a member of the Medical Research Council's radiobiological research unit at the Atomic Energy Research Establishment, Harwell, plans to visit in New York and in Bar Harbor, Maine, before returning on 14 April.

A. G. Searle, a member of the Medical Research Council's radiobiological research unit at the Atomic Energy Research Establishment, Harwell, will visit in the United States until 25 April. His itinerary includes Washington, Chicago, Detroit, Oak Ridge, Tenn., Madison, Wis., and Bar Harbor, Maine.

Robert Oppenheimer, head of the Institute for Advanced Study in Princeton, N.J., has been named to the new Facundo Bueso Chair of Science at the University of Puerto Rico during the spring semester of 1961 for a series of lectures.

Jean Mayer, associate professor of nutrition at the Harvard School of Public Health, has received the Croix de Chevalier de la Légion d'Honneur for his services with the Free French Forces during World War II.

The Department of Agriculture has announced the retirement of **Clarence W. Beebe**, who has worked for the department in the field of leather chemistry since 1929. Since 1941 Beebe has worked at the Agricultural Research Service's Eastern Utilization Research and Development Center in Wyndmoor, Pa.

William L. Stern, assistant professor of wood anatomy at the Yale University School of Forestry, has been appointed curator of the division of woods at the Smithsonian Institution, Washington, D.C.

René J. Dubos will receive the $5000 Passano Award for 1960 on 15 June, during the American Medical Association's convention in Miami Beach. Dubos was selected for the Passano Foundation's award for his studies in bacteriology and biochemistry, and particularly for his work on specific enzymes and on preparation of antibiotics from soil organisms.

Ralph E. Gibson, director of the Applied Physics Laboratory of Johns Hopkins University, Silver Spring, Md., received the fourth annual Captain Robert Dexter Conrad Award during a symposium sponsored by the Office of Naval Research. Gibson was cited for his research contributions for the Navy in the technology of solid-propellent unguided rockets and the development of guided missiles.

Carlo Salvetti, director general of the Italian nuclear center on Lake Maggiore, has been appointed director of the International Atomic Energy Agency's division of research and laboratories.

Robert E. Macherey, senior metallurgical engineer at the Argonne National Laboratory, has been appointed associate director of the metallurgy division. He succeeds **James F. Schumar**, who has resigned to accept a post in industry.

Ramond C. Waddel, formerly with the Naval Research Laboratory, is now a consultant on satellite and rocket instrumentation to the Goddard Space Flight Center of the National Aeronautics and Space Administration, Washington, D.C.

Richard C. Mockler has been appointed chief of the new section on atomic frequency and time standards of the National Bureau of Standards. For 4 years he has directed the project, which has now been given section status.

George Ivanovics, professor of microbiology and director of the Institute of Microbiology at the Medical University in Szeged, Hungary, has been appointed to a visiting lectureship in the department of bacteriology and immunology of the Harvard Medical School.

Landis S. Gephart, chief of the exploratory research and reliability branch of the technical operations division of the Advanced Research Projects Agency, has been appointed director of the reliability and systems analysis office of the National Aeronautics and Space Administration.

Warren H. Lewis, an investigator in cell biology and a retired member of the Wistar Institute, Philadelphia, Pa., has been awarded the triennial Harrison Prize of $1500 by the International Society of Cell Biology. He will attend the International Congress of Cell Biology in Paris, 4–9 September, to receive the award.

Recent Deaths

Roy Chapman Andrews, Carmel, Calif.; 76; explorer and naturalist; former director of the American Museum of Natural History; first discoverer of dinosaur eggs in the Gobi Desert; 11 Mar.

Arthur Louis Day, Washington, D.C.; 90; former president of the Geological Society of America; former director of the geophysical laboratory of Carnegie Institute of Technology; 10 Mar.

Max de Barros Erhart, São Paulo, Brazil; 60; professor emeritus of anatomy at the University of São Paulo; 23 Feb.

Hermann O. L. Fischer, Los Angeles, Calif.; 71; professor emeritus of biochemistry and former head of the department at the University of California, Berkeley; member of the National Academy of Sciences; did research in the biochemistry of carbohydrates and fats; 9 Mar.

Florence Harvey, New York, N.Y.; 70; former member of the social service department of Presbyterian Hospital, Columbia-Presbyterian Medical Center; worked on the social factors related to cancer; 14 Mar.

Albert R. Johnson, Highland Park, N.J.; 80; professor emeritus of engineering at Rutgers University and former chairman of the department of civil engineering; 15 Mar.

Roscoe L. Pullen, Spokane, Wash.; 45; former dean of the University of Missouri School of Medicine; 5 Mar.

Franklin V. Taylor, Alexandria, Va.; 49; head of the engineering psychology branch of the applications research division of the U.S. Naval Research Laboratory; former psychology instructor at Princeton University; 12 Mar.

Francis X. Wilkie, Fairfield, Conn.; 53; chairman of the biology department of Fairfield University; 8 Mar.

Bruce K. Wiseman, Columbus, Ohio; 63; chairman of the department of medicine of Ohio State University; specialist in hematology and blood diseases; 15 Mar.

Book Reviews

The Poisons in Your Food. William Longgood. Simon and Schuster, New York, 1960. 277 pp. $3.95.

This book is an all-time high in "bloodthirsty pen-pushing." It deals with the important problem of chemical additives but from the bias of the nonscientific, natural food-organic gardening cult—the followers of J. I. Rodale (publisher of *Organic Gardening and Farming, Encyclopedia of Organic Gardening,* and so forth), of Natural Food Associates, Inc. (publishers of *Natural Food and Farming Digest),* and others of the same convictions. Most of the "authorities" named in the book are the cult leaders, their gods, or a few true scientists whose work or expressions have been taken either out of context or out of time and used in such manner that they seem to support the doctrine of the believers. The quoted voices of authority and knowledge on the "scientific facts" include *Time, Police Gazette, Prevention,* and the Bonn correspondent of the *Economist.* The book is an irresponsible bid for wide sales through sensationalism. Indeed, one of the author's own definitions describes well my appraisal of it: "[an effort to] beguile, deceive and defraud the housewife by making her think she is getting something she isn't."

The muckraking in this book employs all well-known methods of the irresponsible purveyors of the sensational. Expenditures for life and hospital insurance, for aspirin, and for medical care are cited as indicating a damaging effect of chemicals in foods! The reader is told that ". . . the nation's health is steadily deteriorating." He also learns that "natural foods . . . have a delicate chemical balance that was established by nature for a purpose. These naturally occurring chemicals exist in their specific proportion for a specific purpose. . . . If a larger or smaller quantity were desirable, the amount established by nature through the evolutionary process would have been larger or smaller." The author repeatedly implies that scientists, whose uncited evidence disagrees with his views, have either been bought off by industry or by government, or that they may be deliberately prejudicing their work and reports because their university receives funds from the Public Health Service or from industry, or even because the scientist hopes that after retirement he may be able to get an industrial job!

The author's fascination with the cult of "natural" versus "synthetic and artificial" is well revealed in his presentation concerning meat. The average steak or roast, he writes, "probably comes from a cow born through artificial insemination, raised with an artificial sex hormone implant in its ear, fed synthetic sex hormones, . . . slaughtered—generally by an inhumane method—and sold as meat." In his association through name-calling, he reaches a high in stating that "possible sexual repercussions" on human beings have been commented on by "Dr. Christian Hamburger of Copenhagen, who helped the ex-G.I., George Jorgensen of New York become 'Christine' and Charles McLeod of New Orleans convert to 'Charlotte'." The device of conjuring up fears of impotence or of feminizing influences is an age-old one for those wishing to oppose science. Among primitive peoples, this device is often employed by the witch doctor to oppose the introduction of effective scientific control or treatment of disease.

Longgood's book will no doubt be welcomed by those who believe with him that the public is the victim of a giant conspiracy joined in by the Food and Drug Administration, the American Medical Association, the "big chemical companies" and, apparently, scientists in general—a charge so ridiculous that it deserves only to be ignored. Longgood's book will readily be recognized for what it is by any scientist with so little to do that he takes time to read it.

Finally, it is to be hoped that the great mass of the American public is sufficiently intelligent and logical to recognize that writers and publishers sometimes fail in their responsibility to provide factual and objective information on important issues of the day despite the availability of authoritative, considered source material from organizations such as the Food and Agricultural Organization of the United Nations (FAO), the World Health Organization of the United Nations (WHO), the Food Protection Committee of the Food and Nutrition Board of the National Academy of Sciences–National Research Council (FPC), the Food and Drug Administration, the U.S. Department of Agriculture, and other responsible bodies.

WILLIAM J. DARBY
Department of Biochemistry, School of Medicine, Vanderbilt University

The Genetic Basis of Selection. I. Michael Lerner. Wiley, New York, 1958. xvi + 298 pp. Illus. $8.

This is Lerner's third book on selection and animal breeding. The first, *Population Genetics and Animal Improvement* (1950), was a conventional treatment of the theory and practice of selection for improved performance in livestock. The second, *Genetic Homeostasis* (1954), was concerned with genetic systems that are refractive to directional selection; it was speculative, imaginative, controversial, and influential. The four year periodicity is maintained with this 1958 volume, whose subject content is much like the first, though with overtones from the second.

The book begins with a general review of population genetics and the inheritance of quantitative traits. There is some discussion of natural selection and evolution, but the main emphasis is on the special opportunities for selection which are available to the animal breeder, such things as progeny or family selection, selection indices, inbreeding and crossing, selection for combining ability, and the eventual hope for some utilization of asexual propagation. There is a full, but (necessarily) inconclusive, discussion of the relative merits of intragroup selection and selection for combining ability in crosses.

Both theory and empirical results are included, the latter drawn largely from the author's wide experience with poul-

try breeding. The subject is approached "without either the naturalists' disdain of or the mathematicians' reverence for statistical formulation." Such a nonmathematical treatment loses some precision of meaning. For example, how does Lerner's "integrated gene pool" differ from an adaptive peak, to use Wright's metaphor? A mathematical formulation is not only more precise, but leads to a deeper examination of the conditions under which such a multidimensional peak could, in fact, exist.

The reader will find this book an excellent guide to the literature, for it is outstanding in the breadth of references cited. I like Lerner's writing style; there are many quotable passages, and every chapter is interestingly written. A most welcome device is that of segregating much of the technical or ancillary information into "boxes" that can be read or omitted according to the reader's taste.

On the whole I would characterize the book as being open-minded rather than critical, imaginative and speculative rather than rigorous. Whether this is regarded as a fault or a virtue will depend on the reader.

JAMES F. CROW
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The Weapon on the Wall. Rethinking psychological warfare. Murray Dyer. Johns Hopkins Press, Baltimore, Md., 1959. xxi + 269 pp. $6.

Twenty years ago, psychological warfare had a ring of excitement about it—at least to a dedicated band of practitioners who saw in it a means of hastening victory over Nazi tyranny and serving the cause of democracy and freedom. It was less than exciting to professional soldiers, more accustomed to put their trust in mortars than in microphones, or to professional diplomats, better accustomed to the discretion of traditional foreign relations than to the blasts of mass propaganda. During World War II, both propagandists (most of whom detested the label) and specialists in military, political, and economic action learned to get on better. They developed rules of thumb to formulate policy and guide action, even if they did not leave a full legacy of agreed doctrine.

But today, as Murray Dyer points out, psychological warfare is neither exciting nor adequate as a concept to cover the requirements for persuasion appropriate to cold war or to peace. He proffers "political communication" as an already well-fashioned weapon on the wall, waiting only to be taken down and used in the service of democratic values. But he documents the difficulties, noting misconceptions rife in government officialdom and among other wielders of power, let alone intellectuals, about the nature and needs of political communication. He notes the absence of doctrine. He traces out disagreements between departments of the government (State and Defense especially) about who should wield this weapon and how, in war or in peace. He calls for concerted action under the wise and dramatic leadership of a President standing above departmental parochialism and conflict, aided by a co-ordinator in the White House. He insists that we must match ideas harmoniously with policies and actions, but claims we have not "found our ideas."

Dyer has gone through the immediately pertinent literature comma by comma including the writings of practitioners and scholars and the reports of British and American government committees. No mean practitioner himself, he has interviewed key figures in earlier programs. He has enriched his report by inspecting histories still officially secret. His account and his conclusions gain weight thereby.

But his book is, in some respects, disappointing. He repeats data and reasoning from chapter to chapter as he deals with these themes from slightly different standpoints. He occasionally misconceives as well as misquotes. More serious, from the standpoint of the scientifically trained reader, are his shortcomings in dealing with the fundamental questions of method and in argumentation from evidence. While no one could quarrel with his demand that more science be brought to bear on the intelligence and evaluation functions in communication, his book offers little guidance on how this might be effectively accomplished. The social scientists are already doing somewhat better than Dyer seems to think—notably in analyzing the effectiveness of the operations of the United States Information Agency.

Dyer is more concerned with the developments to date than with the future. He does not attempt to envision the future as a context for judging the appropriateness of political communications. He does not even try to specify the main parameters of limited war, let alone a period after a thermonuclear exchange, in which a more effective political communication must play its role.

Despite understandable shortcomings, this book contains much of value. It provides technical data of interest to sociologists and political scientists, especially those concerned with changes in government structure and bureaucratic behavior under stress.

But far more important is the author's demonstration of the ranges of concern forced on us as a nation by the conscious attempt to use political communications in the service of national values. Despite any shortcoming in philosophical insight or in research method, this book—in particular, its preface by George S. Pettee—makes it impossible for a reader to remain indifferent to the depth of the issues of who we are, what we stand for, and how we express ourselves to ourselves and to the world. These are not technical matters.

What we do about these issues is up to us. No scholar, by demanding presidential leadership of a national program, can impose unity in word or program in a pluralistic, democratic society. Our political communicators will have to be satisfied with something else. Could that be a humble and honest reportage of the many ways in which we show respect appropriate to the condition of many kinds of human beings, or in which we fail to do this? Could that include demonstration of how we use wealth and strength and human energy in the service of human dignity?

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Rand Corporation, Santa Monica, California

Scientific Research in British Universities. 1958–59. Department of Scientific and Industrial Research. Her Majesty's Stationery Office, London, 1959. xii + 466 pp. Paper, £1 5s.

This volume provides brief notes on scientific research in progress in British universities and university colleges and describes the projects in sufficient detail to indicate the scope of the research.

The arrangement of the institutions and of each section within an institution is alphabetical. The head of the department and members of the permanent staff engaged in supervising research are listed. Alphabetical name and subject indexes are included.

The Leopard's Spots: Scientific Attitudes toward Race in America, 1815–59. William Stanton. University of Chicago Press, Chicago, Ill., 1960. 245 pp. $4.

This study is centered around the "American School" of anthropology and ethnology which arose in the antebellum period. The most distinguished member of the group was Louis Agassiz, the Swiss-born naturalist, but the leading figures in developing the school were Samuel G. Morton of Philadelphia and Ephraim G. Squier, whose genuine contributions to the development of anthropology have been largely obscured by the fallacious conclusions drawn from their researches. The group supported a theory of polygenesis for the races of man and held that the divisions between the races were permanently established at their creation. Furthermore, in the "Scale of Nature," these races of man were ordered with the Caucasian at the top and the Negro at the bottom.

Stanton does a splendid job of placing the scientific problem in its social and intellectual context. He shows the character of the men involved in attempts to find a quantitative measurement of racial diversity, the inadequacy of the existing scientific knowledge, the social pressures working on scientific judgments, the interplay of research findings and attitudes toward race, and the ultimate failure of the school following the publication of Charles Darwin's *Origin of Species*. Although Stanton feels that the anti-Biblical crusade that accompanied the teachings of the school neutralized its support of racism among the slaveholders, one cannot help wondering how much of its "scientific proof" of the biological inferiority of the Negro filtered down into the popular mind of the post-Civil War period. Obviously, this book will interest anyone concerned with the background of the color line in the democratic ethos.

On the whole, the American school was a scientific failure, a blind alley, but in selecting it for study, Stanton has been better able to illustrate the complexity of scientific advances. In spite of fallacious conclusions, the work of the school on the nature of hybrids, the transmission of racial characteristics, the contradictions in the environmentalistic theories of biological change, and the inadequacies of the received Biblical account of the creation of races, helped focus attention on vital problems which had to be faced by the Darwinians. The school also helped in the subtle process of transforming the climate of opinion toward an active interest in the problem of racial origins.

Without wishing to quibble over minor points of interpretation, Stanton does attach considerable importance to the idea of *economy* in nature, meaning the frugality of nature, as a part of the outlook of the period (for example, pages 99, 109, 133), and he attributes the recognition of nature's "shocking waste" to Darwin (page 133). The word *economy* was commonplace in the language of natural religion and meant design. The design could include enormous wastefulness as well as frugality, and after the graphic pictures of fossil strata presented by Cuvier, the wasted productions of nature were widely acclaimed. Tennyson, for instance, in his *In Memoriam*: " 'So careful of the type?' but no. / From scarped cliff and quarried stone / She cries, 'A thousand types are gone; / I care for nothing, all shall go . . .'. "

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Teaching Comprehensive Medical Care. A psychological study of a change in medical education. Kenneth R. Hammond and Fred Kern, Jr. Published for the Commonwealth Fund by Harvard University Press, Cambridge, Mass., 1959. xxii + 642 pp. $10.

The decade of the 50's has been a period of experiment in medical education. A number of medical schools have modified their programs and are trying to evaluate the results. This book is a comprehensive report of one of these experiments.

In 1953, the University of Colorado School of Medicine established a General Medical Clinic designed to teach senior medical students "the techniques and philosophy of comprehensive medical care" (page 4). The staff of the clinic (in Denver, directed by Fred Kern, Jr., associate professor of medicine) collaborated with the behavior research laboratory at the university (in Boulder, directed by Kenneth Hammond, professor of psychology) in planning and executing a 5-year research project designed to evaluate the effects of the program. Although Hammond and Kern were the principal investigators and are now the senior authors, it is evident that their staffs contributed significantly to this very thorough and detailed work. The study and the report emerge as fine examples of interdisciplinary effort.

The subtitle, "A psychological study of a change in medical education," comes closer to describing the book than the title itself. About three-fourths of the volume (including the 60-page appendix) is concerned with details of the research. The reader is likely to learn more about the problems of *how* to study the effects of a change in curriculum than about *what* the effects actually are. Its careful and lucid account of research methods used in the study of parts of the educational process in medicine constitutes the book's major contribution. Particularly appealing are the authors' candid reflections on the difficulties met in the several stages of the research.

The authors are conscious of the dual audience for whom the book is intended: "the medical educator" and "psychologists and sociologists." There is enough material, however, to keep these and other groups (for example, educationists) busy for some time to come. Medical educators will read it; so will psychologists and sociologists interested in medical or professional education. But the book deserves a wider audience. Any scientist concerned with the measurement of change in human behavior in a specific (educational) environment will find this a very useful reference work.

One is inclined to agree with the authors that the research itself represents a "pioneer effort." It is unusual on two counts: (i) No other medical school has had a part of its program studied so exhaustively (although others have instituted more extensive changes in program). In no other institution, to my knowledge, has the plan to initiate a curriculum change been so closely intertwined with a research program carefully designed to study its effects. (ii) The design of the study followed classical lines, that is, each of three successive senior classes was divided into an experimental and a control group, the former attending the clinic, the latter the usual senior clerkships. The use of experimental and control groups within the same class over several years knows no parallel in experiments in medical education.

As might be expected, the strengths in the design also contributed weaknesses. Problems appeared in the course

of the research; for example, the project's "major crisis" arose when members of one control group demonstrated an unusual partisanship and hostility (page 123). Problems also arose in the analysis of the data: was it "really a control"? (page 154). The authors discuss these and other questions with freshness and freedom. Their observations should be extremely useful to those planning educational research in medical schools and in other settings.

The busy reader will find the "meat" of the work compressed in the 60 pages of Chapters 10, 11, and 12. The authors wisely present this unit (Part 2) early in the book, for it summarizes the main points of the research, findings, conclusions, and implications for the program. Some of the best writing in the book is found in these chapters. After reading the description of the educational program in Part 1, one gets a clear impression of the consequences of the research for the subsequent program. Parts 1 and 2 (about 160 pages) succeed in bringing to life the "Colorado story." In a foreword, Ward Darley (executive director, Association of American Medical Colleges) discusses the Colorado experiment in relation to the changing scene in American medical education.

The educational and research programs were generously supported by the Commonwealth Fund.

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A California Flora. Philip A. Munz and David D. Keck. Published for the Rancho Santa Ana Botanic Garden by the University of California Press, Berkeley, 1959. 1681 pp. Illus. $11.50.

For a number of reasons, this is a very difficult book to review. First, one must emphasize that it is an important and extremely useful contribution to knowledge, and its importance is certainly more general than its title might imply to the uninitiated. Nevertheless, it has some curious failings that should be pointed out for the benefit of the uninitiated, into whose hands it will certainly come.

The only manual on the flora of California which has been available up to now is that of W. L. Jepson, published in 1925. Munz and Keck, then, fulfill a need for an up-to-date account of the flora, and their account has the additional advantages of an excellent introduction, the use of the metric system for measurements, and a useful list of authors who have been concerned with plants in the flora. (However, compared with Jepson's volume, the illustrations are so few as to be essentially insignificant.) For the botanist working in California, the only other sources available are the incomplete *Flora* of Jepson and the unfortunately expensive, four-volume *Illustrated Flora* of Abrams. The introductory sections on geological history (by D. I. Axelrod), on recent geological history and the vegetation, and on California plant communities are worth reading in their own right, whether or not one works in California; of course, the treatment of families and genera by specialists will always have scope and interest beyond a purely local one.

On these considerations, the book must be commended to botanists generally. On other counts, however, disappointment must be expressed. The value of the book for teaching is reduced by the paucity of illustrations, the thin paper, and the relatively high cost; probably, however, none of these could have been avoided. Unfortunately an arrangement of families has been used which differs greatly from the Englerian system customarily employed. Admittedly the arrangement, largely due to Keck, is an improvement over the Englerian one, but the fact remains that the latter system is still the one employed in the majority of manuals and herbaria. Furthermore, the reasons for the changes that have been made although they may be, to an extent, obvious to an angiosperm taxonomist, are not explained for the beginner.

In the *Flora*, wherever possible, published chromosome numbers are given for the species. This information may be useful for purposes of comparison or for use in selecting interesting problems for future research. However, the numbers given are not always correct, and there is no bibliography, although the references cited presumably may be found in the *Chromosome Atlas* of Darlington and Wylie. Unfortunately, the hypothetical x number (invented by Darlington) is sometimes given instead of the n number (actually determined) —for example, *Hunnemannia.*

Finally, I must mention the rather large number of typographical errors, particularly in the taxonomic section. These range from the transposition of several lines in the keys and descriptions and the inversion of type, to the consistent misspelling of the name of a genus (*Cynanchum*).

Despite these criticisms, more of method than of content, the book will be indispensable to taxonomists and to those concerned with the flora of the Pacific States.

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New Books

Handbook of Electrochemical Constants. Compiled by Roger Parsons. Academic Press, New York; Butterworths, London, 1959. 118 pp. $6.

The Helicopter. Jacob Shapiro. Macmillan, New York, 1960. 269 pp. $4.50.

Isotopic Tracers. A theoretical and practical manual for biological students and research workers. G. E. Francis, W. Mulligan, A. Wormall. Univ. of London Press, London; Essential Books, Fairlawn, N.J., ed. 2, 1959. 545 pp. $8.40.

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Materials and Techniques for Electron Tubes (revised edition of *Materials Technology for Electron Tubes*, 1951). Walter H. Kohl. Reinhold, New York; Chapman and Hall, London, 1960. 658 pp. $16.50.

Mathematics Refresher. Kurt Wolter. Philosophical Library, New York, 1959. 96 pp. $3.75.

Mechanisation of Thought Processes. vols. 1 and 2. National Physical Laboratory, Symp. No. 10. Her Majesty's Stationery Office, London, 1959 (order from British Information Service, 45 Rockefeller Plaza, New York). 990 pp. $9.29.

New and Nonofficial Drugs. 1960. Lippincott, Philadelphia, Pa., 1960. 796 pp. $3.35.

Nuclear Power Plant. E. Openshaw Taylor. Philosophical Library, New York, 1959. 191 pp. $7.50.

Polysaccharides in Biology. Transactions of the fourth conference. George F. Springer, Ed. Josiah Macy, Jr. Foundation, New York, 1959. 326 pp. $5.95.

Principles of Paleobotany. William C. Darrah. Ronald, New York, ed. 2, 1960. 302 pp. $6.50.

Radioisotope Techniques. Ralph T. Overman and Herbert M. Clark. McGraw-Hill, New York, 1960. 492 pp. $10.

La Science et la Théorie de l'Information. Léon Brillouin. Masson, Paris, 1959. 402 pp. F. 4800.

The Story of a Tlingit Community. A problem in the relationship between archeological, ethnological, and historical methods. Bureau of American Ethnology, Bulletin 172. Frederica de Laguna. Smithsonian Institution, Washington, D.C., 1960 (order from Supt. of Documents, GPO, Washington 25, D.C.). 264 pp. $2.

Subsurface Mapping. Margaret S. Bishop. Wiley, New York, 1960. 207 pp. $5.75.

Reports

Demonstration of the Influence of Stimulus and Response Categories upon Difference Limens

Abstract. Representative types of stimulus and response categories were used with the same subjects in determining the difference threshold for visual velocity discrimination. The observed interaction between these variables and difference limens was pronounced.

Data have been reported in *Science* (*1*) which indicate that the difference threshold for the velocity of a seen object "passes through a minimum in the 1-to-3 degrees-per-second region of the range of initial velocities." In subsequent research (*2*) concerned with various aspects of differential velocity and acceleration judgments, an interesting demonstration of the influence of type and number of stimulus and response categories upon threshold magnitude has been observed. That such an interaction exists is, of course, well known to experimental psychologists. We know, however, of no other study in which comparative data have been gathered systematically for the particular categories here employed, and in which the same subjects were used throughout.

The procedure was as follows: The subject was seated in front of a cathode-ray tube (P11) and regarded a 1.5-in. "window" cut into an opaque material superimposed upon the face of the tube. A chin-rest assured a constant (binocular) viewing distance of 10 in. After presenting a ready signal, the experimenter initiated movement of a stimulus spot across the median plane of the window, from just beyond the left to beyond the right edge. The particular velocity of the spot was preset by the experimenter. Approximately 3 seconds after termination of this traverse, a second spot, having the same or different velocity, was similarly flashed across the window. Before each presentation, the subject was told whether the exposure was to be a "standard" or a "comparison." His task was to compare the latter with the former, using one of the sets of response categories shown in Fig. 1, the particular set having been previously specified by the experimenter. For sets I and II, each of the six standards was compared ten times with each of five comparison stimuli: two faster, two slower, and one equal. For example, the slowest standard, 20.06 minutes of visual angle per second, was compared with stimuli of 17.83, 18.95, 20.06, 21.17, and 22.29 min/sec; the fastest standard, 512.71, was compared with stimuli of 334.38, 423.54, 512.71, 601.88, and 691.04 min/sec. (Preliminary trials with each of the standards assured the selection of comparison values which ranged from values which were almost never to values which were almost always judged correctly.) Each standard, then, was compared a total of 50 times with other stimuli. In the case of set II, the subject was not permitted to make a response of "equal," even though equal velocities were presented. For sets III*a* and III*b*, only "faster" and "equal" stimuli were shown, and the only responses permitted were "faster" and "equal."

In set III*a*, the same total number of choices (50) was maintained as for sets I and II, this being accomplished despite the omission of the two slower comparison stimuli by adding a corresponding number of comparison stimuli of the same value as the standard. Thus, for this set, the standard 20.06 was compared with 20.06, 21.17, and 22.29; but three-fifths (or 30) of the total trials consisted of 20.06-versus-20.06 presentations. For set III*b*, the number of standard-versus-standard presentations was kept to ten, the number of such presentations being thereby equated with the number of presentations of each of the two comparison stimuli (a total of 30 trials).

Partial counterbalancing of practice effects was accomplished by having each of the four subjects start the experiment with a different set of response categories. Upon completion of half the total required judgments on a randomly selected standard, each subject was shifted to another standard, as well as to another response-category set. Comparison stimuli were randomized within each of these blocks of trials. At no time were the subjects informed of the accuracy of their judgments. Incremental thresholds were computed for each subject, according to the graphic *z*-score method (*3*). These thresholds were then averaged, with the results shown in Fig. 1.

The generally consistent separation of these threshold functions, as well as the approximately fourfold disparity between the "worst" judgments (set I) and the "best" judgments (set III*b*) are quite striking, although—as previously noted—not entirely surprising (*4*). Less obvious are the precise reasons for these findings. However, we can specify several factors which, taken together, are probably interacting to produce the effects depicted in Fig. 1. These include (i) the number and type of response categories available; (ii) the number and type of stimulus choices available; (iii) the correspondence between i and ii (for example, to gather the data of set II, the "forced choice" technique was used, in which, although slower, equal, and faster stimuli are presented, only the responses "slower" and "faster" are permitted); and (iv) the influence of serial or "expectancy" effects.

It may be noted that set III*b* has response categories similar to the category previously reported (*1*), as well as standard stimuli included in the range of values examined in the earlier work. Despite this, the thresholds are much lower in the present study. This may merely reflect the use of different or of more practiced subjects or a lesser number of comparison stimulus values in the 1957 study (two, as opposed to five).

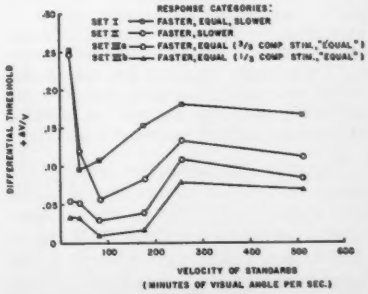

Fig. 1. Incremental difference threshold for the velocity of a seen object as a function of velocity. The parameter consists of various stimulus and response categories.

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

It is possible, however, that the disparity is due to the method of stimulus presentation. In the earlier study, a 3-inch frame was used, the standard being shown for the first half of the frame, the comparison for the second. The increment in velocity (if any) was added instantaneously at the center of the oscilloscope face. Accordingly, the actual acceleration (visually, the "jerk") provided a cue, in addition to the disparity in isometric traverse time. One intriguing speculation is that these subjects may have treated the problem as one requiring a judgment concerning the presence or absence of "jerk," instead of as a comparison of two velocities or two traversal times. Behaviorally, the psychophysical judgment for the subjects in the earlier tests may have been that for the absolute threshold of acceleration instead of that for the difference threshold of velocity, even though mathematically the two stimulus conditions are, of course, equivalent.

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4 December 1959

Recognition of Paired Trigrams as a Function of Associative Value and Associative Strength

Abstract. The accuracy of visual recognition of tachistoscopically presented paired nonsense trigrams was shown to vary directly with the associative value and associative strength. Parallel results were previously reported for meaningful verbal material. It is argued that the same perceptual processes underlie the learning of nonsense and of meaningful material.

Perceptual processes that apply to the visual recognition of meaningful words (*1*) should apply also to the recognition of nonsense trigrams. This report shows that verbal performance is related to accuracy of recognition of nonsense verbal material, as measured by associative value and associative strength (*2*).

Associative value refers to the degree to which a trigram composed by a consonant, a vowel, and a consonant suggests words within a given period of time—for example, 30 seconds. The value of each nonsense trigram is determined by the percentage of subjects who associate meaningful words to it, the value ranging from 0 to 100 percent. Associative value, therefore, is a measure of response evocation.

Associative strength refers to the degree of bond linking a stimulus trigram to a response trigram. The degree of bond and consequent ease of learning of a pair is determined by a variety of intercorrelated measures needed to meet a criterion of learning, usually one errorless repetition of a list of such paired trigrams presented on a memory drum. The most frequently used measures of associative strength are number of trials or of correct responses to criterion. Both measures are highly correlated with each other (mean $r = .94$) and both are related inversely to the associative strength of each pair of trigrams (*2*). Associative strength, therefore, is a measure of response acquisition or of response strengthening for both the stimulus and response terms of a paired trigram.

The relationship between associative value and associative strength of paired nonsense trigrams has been summarized as being nonlinear and nonadditive. Associative strength increases geometrically as the associate values of each pair increase. The higher the associative values of the stimulus and of the response, the easier it is to learn the pair of syllables—that is, the smaller the number of trials to the criterion of learning (*2*). This relationship is shown in the first part of Table 1.

To find how visual recognition of paired nonsense trigrams is related to associative value and to associative strength, the procedure outlined by Heron (*3*) was followed: three lists of 116 pairs of nonsense syllables were typewritten in upper case letters. One trigram was presented to the left of a fixation point, the other to the right. Thus, six letters were shown at a time.

Table 1. Associative values, associative strength, and mean number (± standard deviation) of correct recognitions for paired nonsense trigrams (consonant, vowel, consonant).

Associative values		Associative strength (No. of trials to criterion)	Correct visual recognitions (No.)		
Left	Right		Left	Right	Left + Right
100	100	13.43 ± 6.56	2.38 ± .34	1.63 ± .52	4.01 ± .68
0	100	16.81 ± 4.61	2.06 ± .39	1.44 ± .60	3.50 ± .75
47	47	19.00 ± 6.84	2.01 ± .37	1.33 ± .58	3.34 ± .81
100	0	22.47 ± 5.65	2.19 ± .46	1.34 ± .52	3.53 ± .71
0	0	34.57 ± 9.53	1.96 ± .41	1.21 ± .40	3.17 ± .63

The pairs had been originally ranked on the basis of their associative values and their associative strength. Five combinations of associative values for the left and right syllables were used: 100–100, 0–100, 47–47, 100–0, and 0–0 (*2*).

The pairs were projected tachistoscopically on a glass-beaded, white screen that was uniformly illuminated by a 60-watt bulb at a distance of 5 feet. The angle subtended at the retina by both trigrams was 8°26′. The tachistoscope was a Keystone overhead projector furnished with a Keystone No. 4 universal flashmeter set to expose both syllables simultaneously at 1/100 second. The exposure time for each pair was kept constant throughout the three sessions (one session for each of the three lists). The intensity of the light was adjusted during the presentation of five practice pairs of trigrams that preceded the presentation of the experimental pairs.

The order of presentation within each list was reversed from subject to subject and from session to session to counterbalance serial and fatigue effects. After the presentation of each pair, the subjects were asked to spell out the materials presented to them immediately from left to right, even if they had to guess.

The subjects were 15 men and 15 women from the professional staff of a medical institution. The ages of the men ranged from 22 to 32 years. The ages of the women ranged from 20 to 50 years. Most subjects were in their late 20's. None reported gross visual defects. Ten men and eight women wore corrective glasses.

The results of mean correct visual recognitions as a function of the associative values and associative strength of the trigrams are summarized in Table 1. The mean number of correct recognitions decreases from four out of six for 100–100 associative values to slightly more than three per pair for associative values of 0–0. An analysis of variance performed on the mean number of total (left plus right) correct recognitions yielded a highly sig-

nificant F-ratio of 15.61 ($p < .01$) for the means of the five combinations of associative values studied. The greatest part of the between-variance (11.87) was caused by a linear component (10.18).

To find the relationship between visual recognition and associative strength, only two lists of nonsense trigrams were used. The pairs of the third list could not be ranked reliably according to associative strength. A first list of 40 trigrams of 47–47 associative values yielded a nonsignificant rank-order correlation between the mean number of correct recognitions and the mean number of trials to criterion ($r = .21$). A second list of 40 pairs (10 pairs for each of the following combinations: 100–100, 0–100, 100–0, and 0–0), ranked according to the mean number of correct responses per pair within ten learning and ten transfer trials (*4*), yielded a positive and significant rank-order correlation with the mean number of correct recognitions ($r = .41$, $p < .05$).

Another factor determining the accuracy of recognition was found to be retinal locus. The mean number of correct recognitions was greater to the left than to the right of the fixation point. These results are similar to those reported by Heron (*3*). He found a more accurate recognition in the left visual field than in the right field, when three English letters were exposed simultaneously rather than successively on either side of the fixation point. Such a "differential recognition" was attributed to "the dominant tendency to move the eyes to the beginning of the line" (*3*, p. 47). In addition to associative value and associative strength as determinants of visual recognition of nonsense trigrams, retinal locus should also be taken into consideration in studying visual recognition of nonsensical and of meaningful materials (*5*).

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3. W. Heron, *Am. J. Psychol.* **70**, 38 (1957); D. O. Hebb, *The Organization of Behavior* (Wiley, New York, 1949).
4. This measure is related inversely to the number of trials or of correct responses to a learning criterion (the mean coefficients of correlation with these measures are −.74 and −.62 respectively).
5. This experiment was conducted during my tenure as U.S. Public Health Service postdoctoral fellow at the Institute for Psychosomatic and Psychiatric Research and Training of Michael Reese Hospital, Chicago, Ill.

21 January 1960

Extracellular Invertase Production by Sexually Agglutinative Mating Types of Saccharomyces kluyveri

Abstract. Unisexual strains of both mating types of *Saccharomyces kluyveri* produce exceedingly high yields of extracellular invertase. Yields generally increase with an increase in the number of sets of chromosomes possessed by the unisexuals. The bisexual forms give small amounts in comparison.

In 1956 Wickerham reported the agglutination of opposite sexes in the yeast *Hansenula wingei* Wickerham (*1*). He coined the term "sexual agglutination" (*2*) to differentiate agglutination by opposite mating types from mutual agglutination. The latter process is a much weaker reaction discovered by Eddy (*3*) in strains of brewery yeasts. Both strains of the mutually agglutinative pair (NCYC 74 and 1109) which we obtained from Eddy proved to be bisexual. Recently, Brock (*4*) presented evidence that sexual agglutination in *H. wingei* is due largely to the presence of a specific protein on the cell wall of one of the sexes that reacts with a specific polysaccharide on the cell wall of the opposite sex. He believes the reaction is comparable to that of antigen with its specific antibody. Sexual agglutination is also known to occur in sea urchins and in a few species of algae and protozoa. In these, likewise, there is evidence of stereospecific reactions.

Wickerham (*2*) has reported the occurrence of sexual agglutination in species of four genera of yeasts. The most highly developed species are in *Saccharomyces*, as judged by the ease with which unisexual diploids and presumably unisexual polyploids are produced, as well as by the copious formation of bisexual tetraploids and presumably bisexual hexaploids and octaploids.

Contrary to the generally accepted belief that yeasts do not produce extracellular invertase, we have shown that certain yeasts actually do produce significant amounts (*5*, *6*). The maximum yields of extracellular invertase from *Saccharomyces uvarum* NRRL Y-972 were 126 units per milliliter in aerobic (shaken) culture and 50 units per milliliter in anaerobic (still) culture. Enzyme activity was measured by the procedure of Sumner and Howell (*7*), with slight modification (*6*).

In the present study, bisexual forms of *Saccharomyces kluyveri* Phaff, Miller, and Shifrine (*8*) were found to produce low yields of extracellular invertase commensurate with yields by unisexuals and bisexuals of industrially important species of *Saccharomyces* (*S. cerevisiae*, *S. carlsbergensis*, and *S. diastaticus*). The unisexuals of *S. kluyveri*, however, gave astonishingly high yields (Table 1). Unisexuals 7H1 and 7H2 are two haploids of one sex, and 13H1 and 13H2 are two haploids of the opposite sex. Unisexual diploids 7D1 and 13D1 were derived from 7H1 and 13H1, respectively. Unisexuals designated by code numbers also were largely derived from ascosporic isolates 7 and 13, and are presumed to be triploids and tetraploids. There seems to be a general increase in yields of invertase with an increase in number of sets of chromosomes possessed by the unisexuals. With increasing ploidy the unisexuals also increase in intensity of sexual agglutination; so much so, that presumably unisexual polyploids often show some agglutination by themselves in liquid culture. The ratio of invertase

Table 1. Production of extracellular invertase in aerobic and anaerobic cultures by bisexual and unisexual forms of the sexually agglutinative species *Saccharomyces kluyveri* NRRL Y-4288.

Sex and ploidy	Extracellular invertase	
	Aerobic (unit/ml)	Anaerobic (unit/ml)
Parent bisexual	26	
Bisexual		
Diploid		
7H1 × 13H1	15	
7H1 × 13H2	13	
7H2 × 13H1	13	
7H2 × 13H2	17	
Triploid		
7H1 × 13D1	13	
7H2 × 13D2	14	
7D1 × 13H1	20	
7D2 × 13H2	24	
Tetraploid		
7D1 × 13D1	13	
7D1 × 13D2	24	
7D2 × 13D1	22	
7D2 × 13D2	25	
Unisexual		
Haploid		
7H1	298	
7H2	297	
13H1	366	
13H2	253	
Diploid		
7D1	313	
7D2	337	
13D1	351	
13D2	361	
Presumed unisexual polyploid		
Code 3	792	163
Code 4	491	267
Code 7	295	182
Code 12	340	214
Code 15	380	224
Code 17	340	232
Code 18	279	434
Code 20	580	201
Code 24	325	419
Code 25	385	465
Code 26	492	173
Code 27	230	248
Code 3, transferred serially 2 months	800	
Code 26, transferred serially 3 months	456	

produced aerobically and anaerobically by mating types varies with the individual presumed unisexual polyploid. Both sexes 7 and 13 gave high yields, indicating that invertase is not the specific protein reported by Brock to take part in the sexual agglutination reaction of *Hansenula wingei*. It is to be expected that in addition to the extracellular enzyme, much invertase is bound to the surface of the cells (*6*, *9*).

The isolates designated as code 3 and code 26, of opposite sex, are available from this laboratory. They may be used to demonstrate sexual agglutination and consequent easy harvesting of yeast cells from liquid media. The mating types are grown separately for a few days on YM slants, being transferred daily, and then grown for 48 to 96 hours in shaken flasks at about 28°C in 3 percent glucose or sucrose YM medium, or other liquid media suitable for yeast. The cultures are mixed and sexual agglutination immediately occurs, with rapid settling of the large clumps of agglutinated cells. Codes 3 and 26 are stable. They were transferred daily on YM slants, except for weekends, for 2 and 3 months, respectively, without monitoring, and at the end of this time they gave high yields of invertase (Table 1).

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16 November 1959

Mayaro Virus Isolated from a Trinidadian Mosquito, Mansonia venezuelensis

Abstract. A strain of Mayaro virus has been isolated in Trinidad from the mosquito *Mansonia venezuelensis*. This is the first record of isolation of this agent from naturally infected mosquitoes, caught in the wild.

Mayaro virus was first isolated in 1954 from the blood of a human being working in a forested area of southeastern Trinidad (*1*). The original report recorded the finding of this new agent during August and September in five persons widely scattered over the island. With the exception of the isolation reported here, there had been no further recoveries of this virus in Trinidad through September 1959. However, a survey conducted in 23 representative localities throughout the island has shown that 11 percent of a human population of 615 possess neutralizing antibodies for Mayaro, with localization in southeastern Trinidad, where rates as high as 48 percent were encountered (*2*).

Table 1. Biological properties of two strains of Mayaro virus and of Semliki Forest virus.

Pathogenicity for adult mice	Hemagglutinating antigen from mouse brain	Growth in chick embryo tissue culture
	Semliki Forest	
Highly pathogenic by intracerebral route	Yes	Multiplication and cytopathogenic changes
	Mayaro TRVL 4675	
Not pathogenic by intracerebral route	No*	No multiplication or cytopathogenic changes
	Mayaro TRVL 15537	
Moderately pathogenic by intracerebral route	Yes	Multiplication but no cytopathogenic change

* No hemagglutinating antigen results from acetone-ether extraction of baby mouse brain, but an antigen can be prepared by the sucrose-acetone method.

Mayaro virus is also found in the Amazon valley of Brazil, where it is associated with human illness (*3*), and immunity surveys indicate its presence in the Rupununi savannah and Mazaruni River regions of British Guiana (*2*). We present in this report the first record of the occurrence of this agent in a naturally infected arthropod.

Limited laboratory evidence indicates that mosquitoes are capable of harboring the virus for at least 12 days, and that on one occasion virus was transmitted by the bite of *Aëdes scapularis* (*4*).

Mayaro virus was not recovered from arthropods in this laboratory during 1955 and 1956, although well over 200,000 specimens were ground and inoculated into baby mice. Prior to 1955 this agent would have escaped attention in the entomological work, since only adult mice were then used to receive the original arthropod inocula. Not until March 1957 were we successful in isolating this virus from naturally infected forest mosquitoes. The isolation reported here is the sole Mayaro isolation from mosquitoes, despite the fact that 401,578 mosquitoes were examined in the interval from March 1957 to October 1959.

Mayaro virus was isolated in baby mice inoculated on 28 March 1957 with a suspension from a pool of 49 *Mansonia venezuelensis* (TRVL 15537). These mosquitoes had been taken over a period of 12 working days, between 11 and 27 March, and stored daily as whole insects in sealed ampules at −60°C. They were collected while attempting to bite human beings on the forest floor at our Rio Grande Forest tree station about 7 miles north of Sangre Grande in northeastern Trinidad.

The virus was isolated from the brains of baby mice inoculated intracerebrally with a suspension of this *Mansonia* pool; the agent was established in baby mice and was shown to be filtrable. The virus was reisolated from the original mosquito suspension, both in baby mice and in hamster-kidney tissue culture.

This virus from *Mansonia* is indistinguishable from Mayaro virus (TRVL 4675) by complement-fixation, hemagglutination-inhibition, and neutralization test techniques. Several interesting biological differences among TRVL 4675, TRVL 15537, and Semliki Forest viruses are presented in Table 1 (*6*).

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18 November 1959

Effect of Kinetin on Paramecium caudatum under Varying Culture Conditions

Abstract. When kinetin (1 mg/liter) is added to hay infusion medium, the generation time of *Paramecium caudatum* is shortened immediately upon transfer of the protozoa from stock to isolation culture. Kinetin is particularly effective when culture conditions are suboptimal, perhaps because it substitutes for or supplies some factor which becomes limiting after transfer.

In a previous report, increased rates of cell division in *Paramecium caudatum* were reported after addition of low doses of kinetin (6-furfuryl amino purine) to the culture medium (*1*). In subsequent tests with a new clone of *Paramecium* and new preparations of

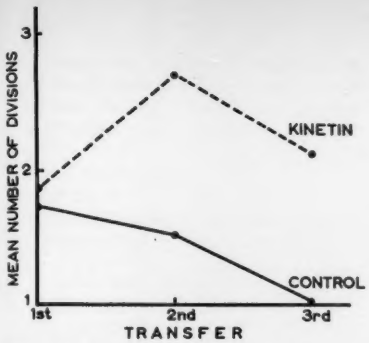

Fig. 1. Cell division in *Paramecium caudatum* under suboptimal culture conditions. The influence of kinetin on division frequencies after each of three daily transfers to new medium.

kinetin, cell division frequencies were also increased, but on a consistently lower level. Because of this lack of reproducibility, our earlier data have been reanalyzed, and these, together with the preliminary results of additional experiments, are presented here.

The results reported in the earlier paper (*1*) were of daily division rates based on the mean of three successive transfers. With 1 mg of kinetin per liter, the ratio of mean divisions in kinetin: mean divisions in control medium ($\bar{k}/\bar{c}$) was 1.45 for the 3-day period, which is highly significant. But when, subsequently, division frequency was recorded for each day separately, an entirely different picture was obtained. Figure 1 shows the original results, separated into the three daily transfers. Kinetin significantly increased the division rate even after the first transfer, but only by 10 percent. The high $\bar{k}/\bar{c}$'s of the following two days (1.78 on the second and 2.19 on the third) were due to two factors: (i) a rising division frequency in the kinetin-containing medium over the 3-day period and (ii) a drop in cell multiplication in the control medium, particularly after the second transfer.

Division of the protozoa after transfer is known to depend on such variables as the amount of food in the new medium, the condition of the transferred animal, the number and kind of bacteria transferred along with the paramecium, and other factors. Depression of division after transfer from stock solution was discussed in an early paper by Chejfec (*2*), while a detailed analysis of the effects of varying culture conditions may be found in a book by Wichterman (*3*).

Multiplication frequencies in our original experiments were low—never more than two divisions in 24 hours. One may assume that some factor necessary for division was suboptimal in these cultures and that this factor was increasingly diluted during the first three transfers. Unfortunately all experiments were terminated after 3 days, and no data are available on whether or when an equilibrium may have been reached.

The following series of tests were conducted a year later, with a new clone of *Paramecium caudatum*, on infusion made from a new batch of hay, and with two new samples of kinetin (California Biochemicals and Waldhof). In these cultures, division frequency per 24 hours was always above 2 and sometimes as high as 5. The experimental procedure was the same as in the previous tests, but daily transfers were made for 6 days. The data given in Fig. 2 are the means of results from ten separate series of tests. In these experiments there was no significant drop in division frequency in the control group. The daily k/c's varied from 1.07 to 1.11, each k being significantly above c at the 5 percent level. Thus even under near-optimal conditions of culture, kinetin could shorten the interdivision period—in these experiments from an average of 8.4 hours in controls to an average of 7.7 hours in kinetin. The constancy of the k/c's over the 6-day period indicates that kinetin does not accumulate in the protozoan, either to increase its stimulatory effect with time or, conversely, to reach an inhibitory level.

A subsequent experimental series was designed to ascertain the presence—or absence—of a stimulatory effect of 1 mg of kinetin per liter during the latter half of the interdivision period. Sixty paramecia were isolated from stock solution, randomized, and divided into groups of 30. One group was placed (singly) in control medium, the other in kinetin-containing medium. After 5 hours' incubation, the number of individuals in each group was recorded. This series was replicated five times. Generation time in these cultures (as ascertained by routine examinations) was approximately 12 hours. However, at the end of the 5-hour period only 5.33 percent of the controls had divided, while 12.67 percent of the paramecia transferred to kinetin-containing medium had completed cell division. The t-value for the difference between divisions in control and divisions in kinetin medium was found to be significant at the 0.01 level. These data indicate that kinetin may be of aid in overcoming the lag period following transfer from stock culture to depression slide, perhaps again by providing or substituting for some factor that is limiting under these conditions.

Finally, it should be pointed out that several samples of kinetin have been found to become toxic after about a year, even though they have been tightly closed and refrigerated. Also, bioassays of equal doses of California Biochemicals and Waldhof kinetin have given dissimilar results, the German preparation having a lower stimulatory action. Lack of uniformity in the effects of kinetin on microorganisms has also been reported by Braun (*4*), who ascribes some of the variability to an influence of high levels of amino acid and trace metal on the action of kinetin (*5*).

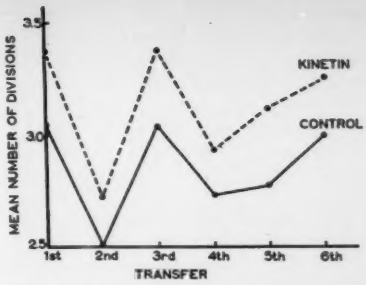

Fig. 2. Cell division in *Paramecium caudatum* under favorable culture conditions. The influence of kinetin on division frequencies after each of six daily transfers.

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12 October 1959

Detection of an Anaplasma marginale Antibody Complex Formed in vivo

Abstract. A naturally occurring anaplasma-antibody complex was detected by exposing erythrocytes of infected cattle to fluorescein-labeled bovine antiglobulin. This technique revealed not only the classical marginal bodies but also the initial *Anaplasma* bodies in the erythrocytes of acutely infected animals. Singly occurring initial bodies were observed in the erythrocytes of healthy carriers.

Detection of an antigen in Weller and Coons' indirect fluorescent antibody method is accomplished by exposure in vitro to unlabeled antiserum derived from naturally infected animals or to antiserum produced by rabbits after inoculation with the antigen, and subsequent treatment with fluorescein-labeled antiglobulin (*1*). A modification of this technique was described by Mellors (*2*), who demonstrated histological

sites of nephrotoxic antibodies with fluorescein-labeled antiglobulin.

In the present procedure used to detect *Anaplasma marginale*, the antigen-antibody reaction is a naturally occuring process within the blood-vascular system of infected animals. To demonstrate this reaction in vitro, thoroughly washed erythrocytes from infected cattle were exposed to the fluorescein-labeled bovine antiglobulin.

Normal bovine globulin was extracted from the serum of a normal calf, according to the method described by Dubert (*3*), which involves the precipitation of globulins with methanol. The antiglobulin was produced by injecting a rabbit three times a week for 3 weeks with 0.5 ml of normal bovine globulin in increasing concentrations (20, 40, and 80 mg). The globulin was precipitated from the antiserum and conjugated to fluorescein isothiocyanate, according to the method of Riggs *et al.* (*4*). To minimize nonspecific fluorescence, the conjugated material was absorbed with powdered rabbit liver prior to use. The technique of fixation and staining of blood films with labeled bovine antiglobulin was similar to that used by Ristic *et al.* (*5*) for detection of *A. marginale* by means of specific fluorescein-labeled antianaplasma antibody.

By the method described above it was possible to observe *A. marginale* growth forms, including marginal and initial bodies (*6*). It was also possible to detect a single initial body occurring within erythrocytes of carrier animals. As viewed with a microscope by ultraviolet light, the initial bodies appeared as punctiform, brilliant foci scattered throughout the erythrocytes of acutely infected animals (Fig. 1) or appeared singly within the erythrocytes of carrier cattle (Figs. 2, 3). The classical *A. marginale* occuring in acute infections appeared as brilliant yellow-green, sharply defined, round bodies (Fig. 1). In contrast, the erythrocytes were clearly seen as grayish-green background structures.

With this technique it was possible to demonstrate the presence of initial bodies not usually observed by conventional staining procedures. This is apparently due to the formation at the site of the organism of a specific, fluorescent-complex aggregate of sufficient size to be observable by microscope. While further evaluation of this technique with regard to its accuracy in detecting anaplasmosis carriers is needed, the principles upon which it is based offer a means of developing a serological test capable of revealing the organism rather than serum antibodies in the blood of anaplasmosis carriers.

It seems reasonable to believe that by the technique described above, other organisms capable of invading the blood-vascular system could be detected. In addition, this method may prove useful in demonstrating *Vibrio fetus, Trichomonas fetus, Leptospira* species, and other microorganisms which, after an acute stage of infection, may persist for long periods in extravascular localities of the body. In these areas complex cell systems apparently are capable of producing antibodies in local tissue that, when combined with specific antigens, offer the prerequisite for carrying out the technique described (*7*).

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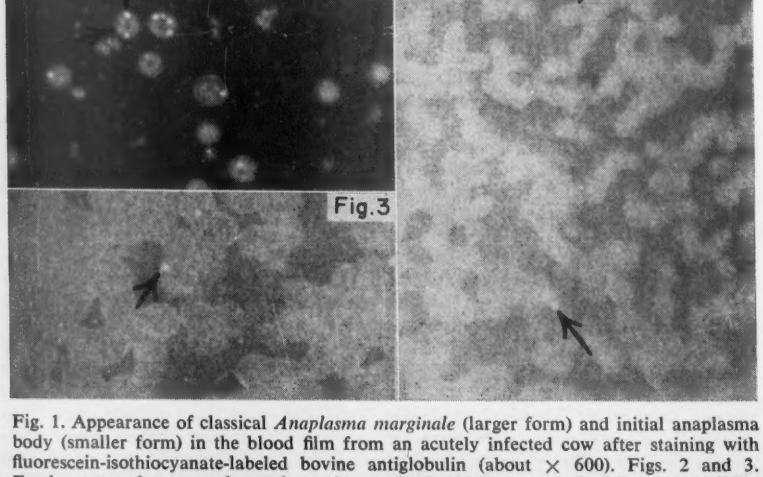

Fig. 1. Appearance of classical *Anaplasma marginale* (larger form) and initial anaplasma body (smaller form) in the blood film from an acutely infected cow after staining with fluorescein-isothiocyanate-labeled bovine antiglobulin (about × 600). Figs. 2 and 3. Erythrocytes of two anaplasmosis carriers, stained with fluorescein-isothiocyanate-labeled bovine antiglobulin. Note the presence of three initial anaplasma bodies (about × 900).

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27 November 1959

Significance of the Presence of Exchangeable Magnesium Ions in Acidified Clays

Abstract. Magnesium ions (Mg^{++}) were shown to constitute a substantial percentage of the total exchangeable cations in acidified clay samples from various sources. This finding helps in solving several problems of long standing in the fields of soil chemistry, soil formation, and geochemical weathering.

Many investigators have concluded that the acidity of naturally occurring soils and artificially acidified soils and clays is due mainly to the presence of exchangeable Al ions rather than to exchangeable H^+ (*1*). It is postulated that after the exchange of the basic cations with H^+ ions, the H^+ ions disappear from the exchange positions and are replaced by Al ions which are either a part of the interior of the crystal lattice (tetrahedral or octahedral positions) or part of free $Al(OH)_3$ and Al_2O_3 which are present in the soil or clay as impurities. Several investigators, however, were aware that the H^+ ions are replaced not only by Al ions but also by Mg^{++} ions (*2*), but the factors affecting this replacement and its significance were only studied recently. The present report is a result of this study.

Briefly, the relative amounts of Mg^{++} and Al^{+++} ions were found to depend on the total MgO and Al_2O_3 contents and the crystal structure of the acidified minerals and on the nature of the acidifying solution and the technique of acidification.

Table 1 is an example of the kind of results obtained for clay minerals and soil clays of varying MgO content acidified by very dilute (0.05 to 0.1*N*) HCl at room temperature. It is clearly seen that in many of the samples exchangeable Mg^{++} ions are even more abundant than exchangeable Al ions. It is also important to note that the Al ions have undergone varying degrees of hydroxylation.

The finding that Mg^{++} is an important exchangeable ion in acidified clay minerals and soils is of great significance to students of soil chemistry and pedology, for it sheds light on several important problems of long standing in these fields.

1) It explains the occurrence of the high percentage of Mg^{++} saturation commonly found in solonetz soils—soils which are formed by leaching sodium saline soils (*3*). Due to the high *p*H of such soils the exchangeable H^+ gained by the soil through hydrolysis tends to be replaced mainly by Mg^{++} rather than Al^{+++}. Crystal lattice Al^{+++} which may be replaced by H^+ would tend to precipitate as $Al(OH)_3$ rather than remain as an exchangeable ion.

2) It explains the occurrence of the high percentage of Mg^{++} saturation and nearly neutral *p*H of soils developed from serpentine rocks in areas where other soils developed from other types of rocks are quite acid (*4*). Furthermore, the finding suggests that the relative composition of the exchangeable ions in soils is strongly influenced by the total composition of the clay minerals and possibly of others. Among California soils formed under similar soil forming factors except parent material, the ratio of exchangeable Mg^{++} to Ca^{++}, the major exchangeable bases, tends to be in the following order: serpentine soils > basaltic soils > granitic soils—the order of their content of MgO (*4*).

3) The observation (*5*) that in some soils, even though they are acid in reaction, the cation-exchange capacity by the NH_4Ac method is lower than the total bases replaced by the NH_4Ac may be explained as follows: Due to the weak acid character of clay acidity, clay minerals adsorb H^+ ions during the leaching process even though the solutions are neutral. These adsorbed H^+ ions enter the crystal lattice and displace Mg^{++} and Al^{+++} ions to the surface. In NH_4Ac the Al^{+++} ions become either hydroxylized to some form of $Al(OH)_x^{(3-x)+}$ or precipitate as $Al(OH)_3$, but the Mg^{++} ions are again replaced by NH_4^+, and thus they appear as replaceable ions which were not on the surface in the first place. This conclusion raises doubts as to whether the method of determining the naturally occurring exchangeable ions by replacement techniques yields results which truly represent their distribution, particularly for soils high in total MgO.

Table 1. Exchangeable ions in various clay samples after their sodium-saturated forms were leached with a dilute acid*.

Sample	Clay minerals in sample†	Total octahedral MgO (gm/100 gm)	Total exchangeable cations (meq/100 gm)	Saturation (%)				*X*‡ OH per Al^{+++}
				Ca^{++}	Mg^{++}	$Al(OH)_x^{(3-x)}$†	H^+	
Kerrite	V	28.1	169	0.0	52.0	48.0	0.0	1.20
Hectorite	S	25.9	55.0	0.0	100.0	0.0	0.0	
Bentonite No. 2	M	7.9	111.0	0.0	17.0	83.0	0.0	0.75
Bentonite No. 7	M	4.5	103.0	0.0	12.0	64.0	24.0	0.0
Bentonite No. 5	M	2.9	86.0	0.0	7.0	74.0	19.0	0.0
			Soil clays					
Dubakella	Ch, V	35.5	12.0	0.0	90.0	10.0	0.0	2.00
Maxwell	Ch, M	10.3	61.6	2.2	90.0	7.8	0.0	2.00
Sweeney	V, M	12.0	59.4	2.5	53.5	44.0	0.0	1.12
Rosamond	V, M, Mic	6.7	33.2	33.1	55.8	11.1	0.0	2.00
Yolo	V, M, K	4.3	43.7	2.4	31.8	65.8	0.0	1.23
Holtville	M, K	3.9	43.4	0.0	30.3	69.7	0.0	1.27
Fresno	V, K, Mic	3.5	18.2	3.4	30.9	65.7	0.0	1.75
Cayucos	M	3.0	63.1	1.2	9.5	89.3	0.0	0.87
Huerohuero (solonetz colloid)	M, V		49.3	0.0	17.5	82.5	0.0	1.00

* The dilute acid was 0.05 to 0.1*N* HCl (350 ml per 1.0 gm of sample). The free acid was removed with H_2O before the samples were extracted with neutral 1*N* KCl to determine the exchangeable ions. † Ch, chrysotile; K, kaolinite; M, montmorillonite; Mic, mica; S, saponite; V, vermiculite. ‡ Determined from the total titratable acidity and total Al^{+++} in the KCl extract.

4) The presence of exchangeable Mg^{++} in acidified clays raises doubts as to whether it is possible to prepare 100-percent homoionic saturated clays, other than Mg^{++}, by the common method which involves saturating the clays first with "acid" and then adding in proper amounts the desired cation in its basic form.

5) Due to the presence of exchangeable Mg^{++} in acidified clays it is not surprising to find that in many published titration curves of clay minerals the full cation-exchange capacity is observed at a *p*H between 10.3 and 10.6, since at this *p*H Mg^{++} precipitates as $Mg(OH)_2$.

6) Most significant of all, the finding that adsorbed H^+ ions on clay surfaces or other minerals readily enter the interior of the crystal lattice to displace Al^{+++}, Mg^{++}, and possibly Fe^{++} or Fe^{+++} whenever these are present, suggests a mechanism by which the chemical alteration and breakdown of the silicate mineral proceeds beyond the first stage of hydrolysis. If the H^+ is replaced by Mg^{++} ions, then the Mg^{++} ions can again be replaced by other H^+ ions, and this process can be repeated until all of the Mg^{++} is exhausted. If the H^+ ions are replaced by Al^{+++} ions and the *p*H of the system is buffered, as by CO_2 or organic substances, the Al^{+++} will either precipitate as $Al(OH)_3$ or become hydroxylized to various degrees as $Al(OH)_x^{(3-x)+}$. These ions remain adsorbed on the clay, but they leave several exchange spots free to adsorb more H^+, which again can enter the crystal lattice and replace more crystal-lattice ions. If Fe^{++} iron is present in the crystal lattice it too would be replaced, but as it reaches the surface and comes in contact with dissolved oxygen it becomes oxidized to Fe^{+++}. The Fe^{+++} iron, due to its chemical behavior will become hydroxylized even more readily than Al^{+++}. In the *p*H range of most soils the Fe^{+++} will precipitate as $Fe(OH)_3$. The replacement of Fe^{++}, therefore, would be expected to proceed even faster than that of Al^{+++}, and indeed minerals which contain large amounts of Fe^{++} do weather more rapidly than those which do not.

From a consideration of the geometry of the crystal structure of the clay mineral, it is necessary to conclude that the H^+ ion enters the interior of the crystal lattice as a bare proton rather than as a hydronium ion (H_3O^+).

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18 November 1959

Epinephrine, Norepinephrine, and Acetylcholine as Conditioned Stimuli for Avoidance Behavior

Abstract. Conditioned leg-flexion responses in dogs were developed with electric shock as an unconditioned stimulus and intestinal stimulation or the effects of injections of various drugs as conditioned stimuli. It is concluded that physiological effects can play a role in the development and maintenance of conditioned avoidance behavior.

Many investigators have suggested the importance of physiological correlates of behavior (*1*). However, few workers have considered the possible role of physiological changes as stimuli (*2*). The studies reported here were conducted to determine whether physiological changes produced by *l*-epinephrine, *l*-norepinephrine, acetylcholine, or stimulation of a Thiry-Vella jejunal loop can become conditioned stimuli in avoidance conditioning.

Beagles, surgically prepared with Thiry-Vella jejunal loops, were restrained in a harness in a soundproof chamber. A balloon inserted into the Thiry-Vella loop could be inflated remotely with 10 cm-Hg pressure. The conditioned stimulus was a balloon inflation lasting 2 seconds, terminated by the unconditioned stimulus, a brief electric shock (of intensity sufficient to cause leg flexion) delivered to the left hind leg. After an appropriate number of trials, balloon pressure *alone* consistently produced leg flexion, the conditioned avoidance response.

Other experiments were conducted to determine whether physiological changes produced by pharmacological agents could also act as conditioned stimuli in avoidance conditioning. A polygraph simultaneously recorded respiration, electrocardiogram, and intestinal activity of Thiry-Vella jejunal loops. The electrocardiographic recordings were made with surface electrodes fixed over the heart apex and the right paravertebral line (modified CR_{6L} lead) in order to minimize artifacts from gross body movements (*3*). Two fine polyethylene catheters were inserted into the external saphenous vein of the right hind leg and attached to syringes outside the soundproof chamber. These catheters permitted the injection of *l*-epinephrine, *l*-norepinephrine, or acetylcholine under remote control.

In each conditioning trial, electric shock was delivered to the left hind limb 30 seconds after the start of the injection. Injections were spaced 5 to 10 minutes apart, to allow time for physiological responses to return to normal levels. No stimuli, other than the physiological effects produced by the injected agents, preceded the shock. Preliminary experiments demonstrated that the monitored physiological responses were generally maximal 30 seconds after injection. Leg flexions occurring within the 30-second interval automatically prevented the shock. Prior to conditioning, none of the agents studied produced leg flexion. The effects of *l*-epinephrine (10 μg/kg), *l*-norepinephrine (10 μg/kg), or acetylcholine (20 μg/kg) came to serve as conditioned stimuli for the avoidance response after an appropriate number of training trials.

Figure 1 shows representative curves for the development of avoidance behavior with the various types of conditioned stimuli. For comparative purposes, other dogs were conditioned to an auditory stimulus (tone) as the conditioned stimulus. Differences in rates of acquisition are apparent; however, direct comparison of these rates is limited because the patterns of stimuli vary, due to differences in dose and in the intensity of effects. Figure 2 is a polygraphic recording of a representative trial, showing physiological effects and conditioned leg-flexion avoidance response. Jejunal activity, respiration, and electrocardiograph were monitored for indications of the occurrence of physiological changes and the temporal relationships of these changes to conditioned leg flexion. It was observed that physiological changes consistently preceded the occurrence of the avoidance response.

After dogs had been conditioned to the effects of 10 μg of *l*-epinephrine per kilogram, doses of 1 or 2.5 μg of *l*-epinephrine per kilogram could produce avoidance responses. In a similar manner, dogs conditioned to 20 μg of acetylcholine per kilogram manifested avoidance responses after the injection of 10 μg of acetylcholine per kilogram. Saline, in comparable volumes, was injected on a random basis with all drug injections through the second catheter; saline injections never produced an avoidance response. These control injections eliminate the possibility that local sensation at the site of injection or volume changes acted as the conditioned stimulus. Injections of glucose

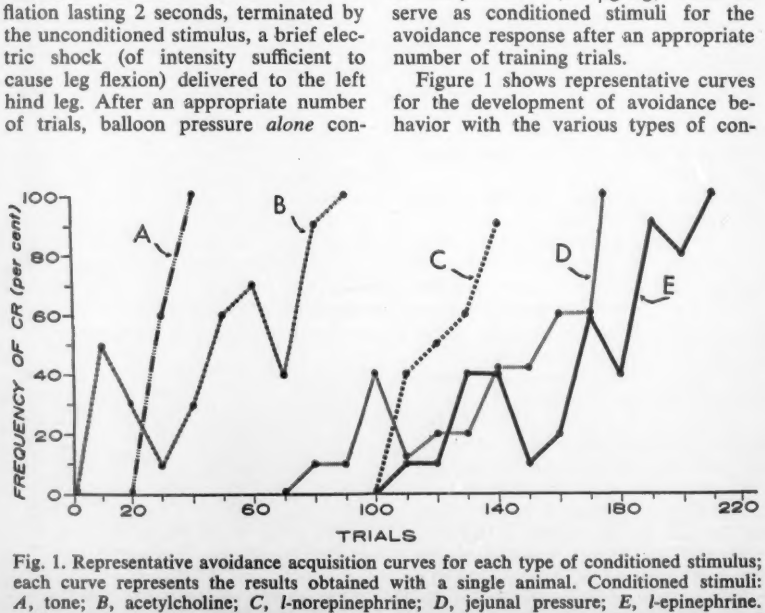

Fig. 1. Representative avoidance acquisition curves for each type of conditioned stimulus; each curve represents the results obtained with a single animal. Conditioned stimuli: *A*, tone; *B*, acetylcholine; *C*, *l*-norepinephrine; *D*, jejunal pressure; *E*, *l*-epinephrine. Different dogs were used in each experiment. Each point represents the percentage of conditioned leg flexions occurring in blocks of ten successive trials.

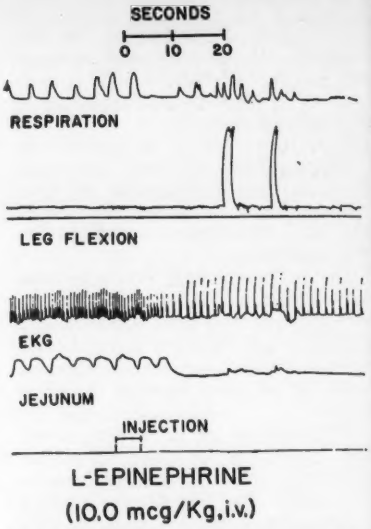

Fig. 2. Polygraphic record showing a trial in which the dog avoided shock. The leg flexion (avoidance response) followed the compensatory bradycardia and decreased intestinal motility produced by the injection of epinephrine.

(5 or 10 mg/kg) also failed to produce conditioned avoidance responses in dogs conditioned with *l*-epinephrine.

This type of conditioning can be due to peripheral physiological effects having an afferent influence centrally, or to direct drug effects on the central nervous system, or to aspects of both (*4*). The experiments involving pressure in jejunal Thiry-Vella loops suggest that peripheral stimulation alone can act as a conditioned stimulus. Physiological effects produced by *l*-epinephrine, *l*-norepinephrine, or acetylcholine can play a role in the development, as well as the maintenance, of a conditioned avoidance response in dogs.

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20 November 1959

Glucose-6-Phosphatase and the Exchange of Glucose with Glucose-6-Phosphate

Abstract. An equation is presented which makes it possible to estimate the exchange activity of glucose-6-phosphatase, as a percentage of the hydrolytic activity, for a given concentration of substrate and acceptor. The quantitative significance of the exchange-inhibition phenomenon is discussed.

Hepatic microsomal glucose-6-phosphatase catalyzes the exchange of glucose with glucose-6-phosphate (G-6-P), and this exchange is correlated with an inhibition of the hydrolytic activity (*1*). The purpose of this communication is to evaluate, in so far as possible, the significance of this exchange-inhibition phenomenon. Neither the exchange nor the inhibition appears to be quantitatively significant for normal hepatic carbohydrate metabolism, but the exchange activity could be important in liver slice experiments of the type described by Cahill *et al.* (*2*) where high concentrations of glucose have been used and where it has been assumed that the conversion of glucose to G-6-P is carried out solely by an enzymatic phosphorylation using a phosphate donor such as adenosine triphosphate.

The exchange activity equals the inhibition of the hydrolytic activity, and the inhibition can be estimated by using Eq. 1 (*3*, *4*) or, for a typical set of conditions, from Fig. 1.

$$\frac{v_i}{v} \cdot 100 = \frac{1+\frac{Km}{S}}{\left(1+\frac{I}{K_i}\right)+\left(\frac{k_2}{k_1}\cdot\frac{I}{K_i'}\cdot\frac{1}{S}+\frac{Km}{S}\right)} \cdot 100$$

where v is the velocity in the absence of inhibitor; v_i is the velocity in the presence of inhibitor; S is the substrate concentration; I is the inhibitor concentration; Km is the Michaelis constant; K_i' is the concentration of inhibitor that gives 50 percent inhibition when $S = \infty$; and k_2/k_1 is the dissociation constant of the enzyme-substrate complex.

It should be pointed out that Km ($6.1 \times 10^{-3}M$), K_i' ($8.8 \times 10^{-2}M$), and k_2/k_1 (8.3×10^{-4}) were determined at *p*H 6.0, which is the *p*H optimum of glucose-6-phosphatase. The assumption that these constants can be used to estimate the exchange-inhibition phenomenon *in situ* (*p*H of 7.4) is based on the observation that the percent inhibition by glucose is constant over a wide *p*H range (*3*). In summary, Eq. 1 can be used to calculate the percentage activity for a given concentration of substrate and inhibitor. The absolute quantity of inhibition (exchange) would also require an estimate of the velocity of the hydrolytic activity (v or v_i).

The inhibition by glucose of hepatic glucose-6-phosphatase in the rat, using the data of Steiner and Williams (*5*) for G-6-P (normal, 0.47 μmole/gm; diabetic, 0.13 μmole/gm) and glucose (normal, 5.9 μmole/gm; diabetic, 30 μmole/gm), would be 1.3 percent in the normal and 5.0 percent in the diabetic. Even if glucose-6-phosphatase were the limiting step in gluconeogenesis, this estimated inhibition does not appear to be significant. Under these conditions the exchange activity would also be unimportant, since the transfer of a phosphoryl group from one glucose to another is of no consequence. Molecules which are structurally related to glucose also act as acceptors and inhibitors, but glucose is the best naturally occurring inhibitor and acceptor. Furthermore, the "glucose analogues," known to be present in liver, are present in low concentrations, and their 6-phosphate esters can be formed more readily by other pathways.

In the presence of C^{14}-G-6-P or C^{14}-glucose, the exchange reaction catalyzed by glucose-6-phosphatase (Eq. 2) would

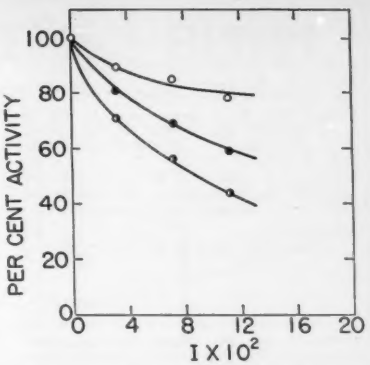

Fig. 1. Inhibition of glucose-6-phosphatase by glucose at different concentrations of G-6-P. The open circles, solid circles, and half-open circles represent data obtained in the presence of $1 \times 10^{-3}M$, $8.0 \times 10^{-3}M$, and infinite G-6-P, respectively. Values at an infinite concentration of G-6-P were obtained from the ordinate intercept of a Lineweaver-Burke plot (reciprocal velocity versus reciprocal substrate concentration). The experimental data used for this figure have been taken from the results for the microsomes prepared from an alloxan-diabetic rat given in Fig. 3 of a previous publication (*3*), but the inhibition data for microsomes from diabetic and normal animals are indistinguishable.

be indistinguishable from the hydrolysis of G-6-P by glucose-6-phosphatase (Eq. 3) or the phosphorylation of glucose by a kinase (Eq. 4).

$$\text{G*-6-P} + \text{Glucose} \rightleftharpoons \text{Glucose*} + \text{G-6-P} \quad (2)$$

$$\text{G-6-P} + \text{H}_2\text{O} \rightarrow \text{Glucose} + \text{orthophosphate} \quad (3)$$

$$\text{Glucose} + \text{ATP} \rightarrow \text{G-6-P} + \text{ADP} \quad (4)$$

The actual net effect of the exchange reaction on the rate of transfer of labeled G-6-P to glucose (Eq. 3) would be negligible, since the transfer by the exchange reaction is effectively canceled by the concomitant inhibition of the hydrolytic activity. The conversion of glucose to G-6-P by the exchange reaction, however, would be an apparent kinase (Eq. 4) and the contribution of the exchange reaction to the total apparent kinase could vary from a minor to a major rate, depending upon the conditions. It also should be emphasized that the net transfer of labeled material per unit time, with an exchange reaction, depends upon the difference in specific activity between the donor and acceptor.

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References and Notes

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2. G. F. Cahill, Jr., J. Ashmore, A. E. Renold, A. B. Hastings, *Am. J. Med.* **26**, 264 (1959).
3. L. F. Hass and W. L. Byrne, *J. Am. Chem. Soc.*, in press.
4. Eq. 1 is the quotient of the steady-state rate equations in the presence and absence of inhibitor for the following mechanism:

$$E + G\text{-}6\text{-}P \rightleftharpoons E - G\text{-}6\text{-}P \rightleftharpoons E - P + \text{Glucose}$$
$$\downarrow H_2O$$
$$E + \text{Orthophosphate}$$

The inhibition of glucose-6-phosphatase by glucose is not one of the classical types, competitive, noncompetitive, or uncompetitive [P. W. Wilson, *Respiratory Enzymes*, H. A. Lardy, Ed. (Burgess, Minneapolis, 1949), pp. 16–56], and is best described by the above mechanism and its rate equation (Eq. 3).

5. D. F. Steiner and R. H. Williams, *J. Biol. Chem.* **234**, 1342 (1959).

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10 November 1959

Mean Lifetime of Free Radical Chains Determined by a Flow Technique

Abstract. When radiolysis is induced in chloral hydrate solution flowing through a glass coil wound around a lead brick and irradiated from one side only, the rate of reaction depends on the flow rate. The effect resembles that for intermittent irradiation and makes it possible to estimate the mean lifetime of the free radical chain.

Intermittent irradiation techniques have been widely used in photochemistry (*1*) and to a lesser extent in radiation chemistry (*2*) for the determination of mean life-times of free radical chains. The usual method of securing intermittency is to employ a rotating sector between the source of radiation and the material being studied. Alternative methods include the reciprocating source method (*3*), in which the source is repeatedly removed from the reaction vessel and replaced in it after a short time interval, and the rotating source method, in which the cells containing the solution to be irradiated are mounted on a wheel which is rotated past an aperture in a shield containing a Co^{60} source (*4*). A somewhat different approach to the problem has been reported by Goldfinger and Heffelfinger (*5*). Radical formation was initiated in a mixture of styrene and benzoyperoxide by exposure to a mercury arc. After the stream of material had passed the illuminated region it fell freely, thus allowing chains to grow. Chains were terminated by means of a picric acid inhibitor, and by varying the distance between the source of radiation and the picric acid solution, the half-life of the styrene free radical chain could be determined.

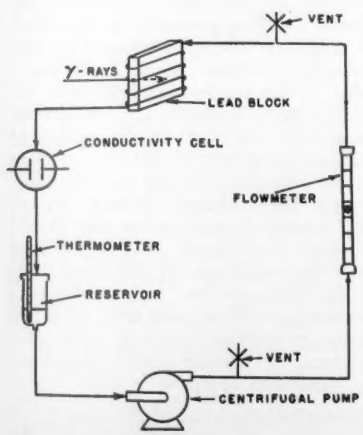

Fig. 1. Diagram of flow system; a 90-c Co^{60} source is housed in the lead cylinder.

Yet another method of achieving intermittent irradiation is to use a flow system in which the material flows past the Co^{60} source in such a way that it receives successive bursts of radiation. A brief description of the application of this method to chloral hydrate solutions follows. The mean lifetime of the free radical chains in 1*M* chloral hydrate solutions had previously been determined to be about 0.1 second by Freeman *et al.* (*6*), who used a rotating sector technique and gamma rays. The corresponding experiments for beta rays have also been reported (*7*).

Fisher U.S.P. chloral hydrate was used without further purification, and the amount of acid formed on irradiation was measured with a conductivity cell (*8*). For details see (*9*).

A 90-c Co^{60} source, housed in a concrete irradiation cave, was used as a source of gamma rays (*10*). The radiation dose was measured with the usual Fricke ferrous sulfate dosimeter and was found to be approximately 640 rad/min. The flow system is indicated diagrammatically in Fig. 1. The solution was pumped by means of a Cole-Parmer polyethylene 1/35 hp centrifugal pump through a flow meter, a reaction cell, a conductivity cell, and a reservoir. The reaction cell consisted of a glass coil of ten turns wrapped around a lead block in such a manner that a given volume element of solution would be subject to alternate "dark" and "light" periods. Flow rates were varied from 10 to 500 ml/min. The inside diameter of the tubing was 5.1 mm. The lengths of tubing exposed ("light" period) and unexposed ("dark" period) were 13.7 and 20.4 cm, respectively. Thus, for a flow rate of 500 ml/min, the "light" and "dark" periods were 0.34 and 0.52 seconds, respectively.

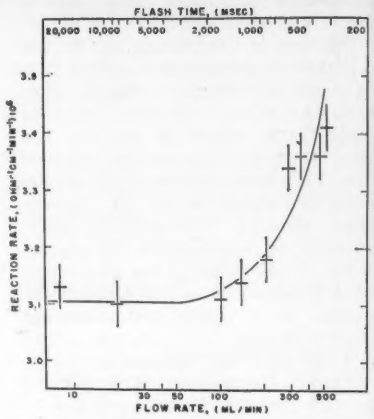

Fig. 2. Change in reaction rate versus flow rate.

Figure 2 shows the change in rate of reaction with rate of flow for 0.5*M* chloral hydrate solution at 25°C and at a *p*H of 4.60. The curve resembles that obtained by the rotating sector method (*6*) and indicates that the irradiation time in the region of reaction-rate increase is comparable to what would be expected if there were a strict parallel between intermittency in space and time. If we assume a complete parallel, the indicated mean lifetime of the chain is of the order of 1/5 second. Because of the dimensions of the reaction zone (4 by 4 in.) and its proximity to the Co^{60} source, the dose rate was not uniform, and thus a more detailed analysis of the experiment is not warranted. However, the results do serve to illustrate the possibilities of the method, which would appear to have wide applicability in both radiation chemistry and photochemistry.

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6. G. R. Freeman, A. B. Van Cleave, J. W. T. Spinks, *Can. J. Chem.* **32**, 322 (1954).
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2 November 1959

Meetings

New Jersey Academy of Science

The New Jersey Academy of Science, a recent affiliate of the AAAS, came into being during the year 1954 through the efforts of a small group of individuals, guided largely by Roger H. Charlier and Courtlandt J. Daley, who were interested in founding an organization that would eventually accomplish for science on a state-wide basis what the AAAS achieves on a nation-wide scale. Such an organization, they felt, could be made to serve all disciplines in science by bringing together at regular intervals scientists, engineers, and members of the medical and dental professions. It would thus effect a degree of cross-fertilization in science which ordinarily is unattainable in professional organizations devoted to specialized fields. It was also hoped that by embracing all facets of science, engineering, and medicine, the organization might be able to bring together and to share the points of view reflected in these areas and to apply them to state as well as to national problems of a scientific nature. It would thus serve not only to unify the scientific interests of the state but also to encourage interdisciplinary study and research. Concomitantly it would, as stated in the constitution, "promote fraternal relationship among those engaged in scientific work."

The first annual meeting was held at Township Hall in Chester, N.J., on 29 January 1955, with about 40 members in attendance. At this meeting several of the members presented papers, officers were elected for a 2-year period, and an executive committee and an editorial staff for the academy publication, the *Bulletin*, were appointed. The executive committee was empowered to draw up a contitution and bylaws. The draft constitution and bylaws, with minor alterations, were approved by the members at the annual meeting held in 1956.

According to provisions of the constitution all officers are elected for a period of 2 years, the newly elected officers assuming their duties at the conclusion of the annual meeting of the election year. Election is by secret ballot, a proposed slate of officers being submitted to the members by a nominating committee appointed by the president. Nominations may also be made from the floor at the time of the election. The present officers, whose terms expire in 1961, are as follows: president, Robert K. Zuck (Drew University, Madison); vice presidents, James H. Leathem (Rutgers University, New Brunswick) and M. Lelyn Branin (Newark College of Engineering, Newark, N.J.); executive secretary, Hirsch L. Silverman (Yeshiva University, New York, N.Y.); recording secretary, Michael Charney (Hackensack Biochemical Laboratory, Hackensack); and treasurer, Louise F. Bush (Drew University). The academy representative on the AAAS Council is M. Lelyn Branin.

The *Bulletin* is published semiannually under the co-editorship of M. Lelyn Branin and Hirsch L. Silverman. Orginal contributions to science are published, together with news items and announcements of particular interest to members. Plans are under way for the creation of a monthly or bimonthly newsletter to supplement the *Bulletin* and thus make more space available in the latter for publication of scholarly contributions.

The annual meeting of the academy is held in March or April of each year, ordinarily on the campus of one of the New Jersey colleges or universities. It consists of a business meeting and technical sessions in the afternoon, followed by an informal dinner and an evening session at which a nationally known scientist speaks on some subject of broad public interest. All sessions except the business meeting are open to the public. This year's annual meeting was held on 25 March at Newark College of Engineering, in conjunction with the college's 75th anniversary celebration.

The academy is now planning to establish, with the cooperation of the New Jersey Science Teachers Association, a junior academy of science for public and private high-school and junior high-school students throughout the state. Committees representing both the academy and the science teachers association have recently held a series of meetings to determine what form the organization should take and how the plan finally agreed upon should be implemented. It is hoped that the details of an organizational plan for the junior academy can be agreed upon before the spring meetings of the two sponsoring societies so that the plan can be presented to the members for discussion and possible approval.

During the past 5 years the academy has grown from the original small group to an organization of approximately 250 members, including some out-of-state and foreign members. Membership is open to anyone who has an interest in science. The annual membership fee of $3 includes a subscription to the *Bulletin*. Student membership is available to high-school and college students for one-half the regular annual fee.

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Newark College of Engineering, Newark, New Jersey

Forthcoming Events

April

27–30. American Meteorological Soc., general meeting with American Geophysical Union, Washington, D.C. (K. C. Spengler, AMS, 45 Beacon St., Boston 8, Mass.)

28–30. American Assoc. of Pathologists and Bacteriologists, Memphis, Tenn. (R. L. Holman, Dept. of Pathology, Louisiana State Univ., School of Medicine, New Orleans)

28–30. American Soc. of Human Genetics, Memphis, Tenn. (W. J. Schull, Dept. of Human Genetics, Univ. of Michigan, 1133 E. Catherine St., Ann Arbor)

28–30. Current Concepts in Medicine, 2nd intern. symp., Philadelphia, Pa. (M. J. Schwartz, Deborah Hospital, 901 Walnut St., Philadelphia 7)

28–30. Midwestern Psychological Assoc., Columbus, Ohio. (I. E. Farber, Dept. of Psychology, State Univ. of Iowa, Iowa City)

29. Parenteral Drug Assoc., Philadelphia, Pa. (H. E. Boyden, PDA, 4865 Stenton Ave., Philadelphia 44)

29–30. Thermonuclear Processes, conv., London, England. (Institution of Electrical Engineers, Savoy Pl., London, W.C.2)

30. Idaho Acad. of Science, annual, Pocatello. (A. E. Taylor, Graduate Div., Idaho State College, Pocatello)

30–2. Society for American Archaeology, Salt Lake City, Utah. (D. A. Baerreis, Sterling Hall, Univ. of Wisconsin, Madison 6)

May

1–2. American Soc. for Clinical Investigation, Atlantic City, N.J. (S. J. Farber, New York University College of Medicine, 550 First Ave., New York 16)

1–5. American Assoc. of Cereal Chemists, Chicago, Ill. (J. W. Pence, Western Utilization Research and Development Div., 800 Buchanan St., Albany 10, N.Y.)

1–5. Electrochemical Soc., Chicago, Ill. (H. B. Linford, ES, 1860 Broadway, New York 23)

1–5. Society of American Bacteriologists, 60th annual, Philadelphia, Pa. (D. M. Cleary, Box 354, Upper Darby, Pa.)

1–5. AAAS Southwestern and Rocky Mountain Div., Alpine, Tex. (M. G. Anderson, New Mexico College of Agriculture and Mechanical Arts, P.O. Box 97, University Park)

2. American Federation for Clinical Research, Atlantic City, N.J. (J. E. Bryan, 250 W. 57 St., New York 19)

2–3. Reactions between Complex Nuclei, 2nd conf., Gatlinburg, Tenn. (R. S. Livingston, Oak Ridge Natl. Laboratory, Oak Ridge, Tenn.)

2–4. Aeronautical Electronics, conf., Dayton, Ohio. (L. G. Cumming, IRE, 1 E. 79 St., New York 21)

2–5. Flight Test Symp., natl., San Diego, Calif. (H. S. Kindler, Instrument Soc. of America, 313 Sixth Ave., Pittsburgh 22, Pa.)

2–11. International Cancer Cytology conf., Mexico, D.F., Mexico. (Office of Intern. Conferences, Department of State, Washington 25)

2–11. Pan American Medical Assoc., cong., Mexico City, Mexico. (J. J. Eller, 745 Fifth Ave., New York 22)

3–4. Association of American Physicians, Atlantic City, N.J. (P. B. Beeson, Yale Univ. School of Medicine, New Haven 11, Conn.)

3–4. Conference of Veterinarians, annual, Philadelphia, Pa. (W. H. Rhodes, School of Veterinary Medicine, Univ. of Pennsylvania. Philadelphia 4)

3–5. Society of Pediatric Research, Swampscott, Mass. (C. D. West, Children's Hospital, Cincinnati 29, Ohio)

3–6. Fuel Element Fabrication, symp., Vienna, Austria. (Intern. Atomic Energy Agency, 11 Kärntner Ring, Vienna)

5–6. American Pediatric Soc., annual, Swampscott, Mass. (A. C. McGuinness, 2800 Quebec St., NW, Washington 8)

5–8. Wilson Ornithological Soc., Gatlinburg, Tenn. (A. M. Bagg, Farm St., Dover, Mass.)

6–7. Minnesota Acad. of Science, St. Cloud. (J. P. Emanuel, Winona State College, Winona, Minn.)

6–7. North Carolina Acad. of Science, Greensboro. (J. A. Yarbrough, Meredith College, Raleigh, N.C.)

6–7. North Dakota Acad. of Science, annual, Fargo. (B. G. Gustafson, Box 573, University Station, Grand Forks, N.D.)

6–7. Population Assoc. of America, annual, Washington, D.C. (K. B. Mayer, Dept. of Sociology and Anthropology, Brown Univ., Providence 12, R.I.)

6–7. South Dakota Acad. of Science, 45th annual, Brookings. (J. M. Winter, Dept. of Botany, Univ. of South Dakota, Vermillion)

6–8. International Cong. of Phlebology, 1st, Chambéry, France. (J. Marmasse, 3, rue de la République, Orléans (Loiret), France)

(See issue of 18 March for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science *nor the writer assumes responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to the manufacturer. Include the department number in your inquiry.*

■ TELEMETERING SYSTEM for transmitting on-off information from remote stations to a central location uses existing telephone lines. Audio tones in the range from 200 to 450 cy/sec are used with adjacent tones spaced 10 cy/sec apart. Interference with normal telephone use is prevented by filters installed at each telephone. These block signal-tone frequencies, but pass voice and ringing frequencies. The equipment is transistorized. Modulators operate on 6 to 8 volts d-c power and demodulators, one for each 10 modulators, operate on 115 volts a-c. Ambient operating temperature range is 10° to 50°C. (Duplex Div., Bell & Gossett Co., Dept. Sci462, Morton Grove, Ill.)

■ RESISTANCE-RATIO INSTRUMENTS are available in three-, four-, and six-dial models. The six-dial model permits comparison of resistors with a standard up to ratios of 10 to 1 with ratio limit of error of ±0.01 percent. Resistance between external terminals is a constant 2,111,110 ohms for all settings. Potential contact may be set at any point to the nearest 0.001 ohm. Ratio range of the 4-dial model is 0.994445 to 1.005555 in steps of 0.000001, and of the 3-dial model, 0.99445 to 1.00555 in steps of 0.00001. (Minneapolis-Honeywell Regulator Co., Rubicon Instrument Div., Dept. Sci463, Ridge Ave. at 35 St., Philadelphia 32, Pa.)

■ LINEAR MOTION POTENTIOMETER is designed to operate over the temperature range −65° to +500°F. Displacement range is 1.31 in. Available resistances range from 1000 to 20,000 ohms with resolutions ranging from 0.0013 to 0.0035 in. Power rating is 4.0 watts at 40°C. (Bourns Laboratories Inc., Dept. Sci447, P.O. Box 2112, Riverside, Calif.)

■ ELECTRON MICROPROBE ANALYZER, for analysis of magnesium and higher atomic number elements, operates by exciting x-radiation from elements in a volume of a few cubic microns on the surface of a sample. Electron-optics system contained in a vertical column includes a biased gun, two magnetic lenses, and alignment devices. Effective focus diameter is of the order of 2 μ when measured in zirconium at 25 kv. Accelerating voltage is adjustable in 5-kv steps from 10 to 50 kv. The specimen stage accommodates samples up to 0.5 in. in diameter and 0.25 in. thick. Translation facilities operate in two orthogonal directions, one horizontal and one inclined 30 deg. X-ray optics are enclosed in a ray-proof chamber containing three or more adjustable curved-crystal spectrographs or one scanning x-ray spectrograph. Helium bags are optionally available. Three counting channels provide for two Geiger counters and one scintillation counter. An optical system is provided for viewing the sample surface at a fixed magnification of 220. The instrument is housed in two units, one a desk console and the other an electronics cabinet. (Philips Electronic Instruments, Dept. Sci461, 750 S. Fulton Ave., Mount Vernon, N.Y.)

■ AMPLIFIER AND SWEEP VOLTAGE GENERATOR are combined in this instrument that provides seven rates of sweep voltage for use with X-Y plotters. Rates range from 0.5 mv/sec to 50 mv/sec, with ±2 percent accuracy. A rate of 0.1 mv/sec can be obtained with some loss of linearity. In its amplifier setting the instrument becomes a chopper-stabilized d-c amplifier delivering either amplified current output or voltage output by use of proper shunts. Used with a standard 10 mv/in. recorder, sensitivities up to 10 in./mv are obtainable. (Houston Instrument Corp., Dept. Sci464, 1717 Clay Ave., Houston 3, Tex.)

■ OSCILLOSCOPE CAMERA records patterns with object-to-image-size ratio 1 to 0.9. Multiple exposures are provided by moving the lens to 11 detented positions while the camera back remains fixed. Lens adjustment can be made without removing the camera from the oscilloscope. The pattern can be observed with both eyes while exposures are being made. (Hewlett-Packard Co., Dept. Sci460, 275 Page Mill Rd., Palo Alto, Calif.)

■ FREQUENCY-TO-VOLTAGE CONVERTERS are available in six standard models covering the audio frequency range to 5000 cy/sec, with models to 14,000 cy/sec available on special order. Standard outputs are 0 to 1 and 0 to 5 volts d-c filtered, 0 to 50 μa filtered, and 0 to 1 ma unfiltered. Both zero setting and range scale are adjustable. Standard units offer linearity and accuracy said to be better than ±½ percent of full-scale frequency with temperature sensitivity less than 10^{-4}/°C from − 60° to +80°C. Accuracies within ±0.1 percent and temperature sensitivity under 5×10^{-5}/°C are said to be available on special order. (Pioneer Magnetics Inc., Dept. Sci469, 5888 Wilshire Blvd., Los Angeles 36, Calif.)

JOSHUA STERN

National Bureau of Standards, Washington, D.C.

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(a) **Neurophysiologist** Ph.D.; research in psychophysiology of stress, sound, vision, altitude responses, teaching and administrative experience; available for top-level academic or research appointment in physiological psychology. (b) **Biology** Ph.D.; teaching-research experience in anatomy, zoology, biology, including radiobiology and radiation medicine; prefers research appointment. (c) **Organic Chemist** Ph.D.; 3 years of science teaching, 8 years of research and clinical biochemistry experience; available for academic, clinical or pharmaceutical appointment. S4-1 Medical Bureau, Inc., Science Division, Burneice Larson, President, 900 North Michigan Avenue, Chicago. X

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Anatomy Department, Dalhousie University, Halifax, Nova Scotia. Applications are invited for two positions at the rank of **Assistant Professor** in the Department of Anatomy. Experience in the teaching of gross anatomy is essential and a willingness to undertake some teaching in neuroanatomy, histology, or embryology desirable. The successful candidates will be expected to participate in the active research program of the department, for which good facilities are available. Applicants with a medical degree will be preferred although those with an honors degree in medical science or biology will receive special consideration. The salary for a medical graduate with minimum qualifications will be $6500. The salary will depend upon qualifications and experience. Applications should be made to Professor R. L. deC. H. Saunders at the above address. 3/4; 4/1; 5/6; 6/3

(a) **Regional Medical Director;** requires demonstrated administrative abilities to represent prominent pharmaceutical company to physicians; liaison, consultant, correspondence responsibility; some travel; to $17,000; Midwest. (b) **Biochemist;** Ph.D. to supervise department; also responsible 30 technical employees in busy, well-equipped laboratory, 250-bed general hospital, expanding; upper Midwest. (c) **Associate Director, Clinical Research;** young M.D. particularly experienced cancer, arthritis, pulmonary, related areas to establish and coordinate clinical testing programs for new products, evaluate results; clinical, medical liaison; to $16,000 with exceptional benefits; prominent company. (d) **Pharmacologist/Physiologist** for animal experimentation and (e) **Analytical Chemist or Biochemist;** for synthetic organic medicinals; expanding consulting firm principally in drug, medical evaluation; salaries commensurate ability; excellent advancement opportunities. (Please write for an analysis form. Our 64th year: Founders of the counseling service to the medical profession; serving medicine with distinction over half a century.) Science Division, Woodward Medical Bureau, Ann Woodward, Director, 185 North Wabash, Chicago. X

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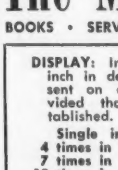

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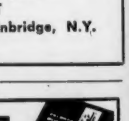

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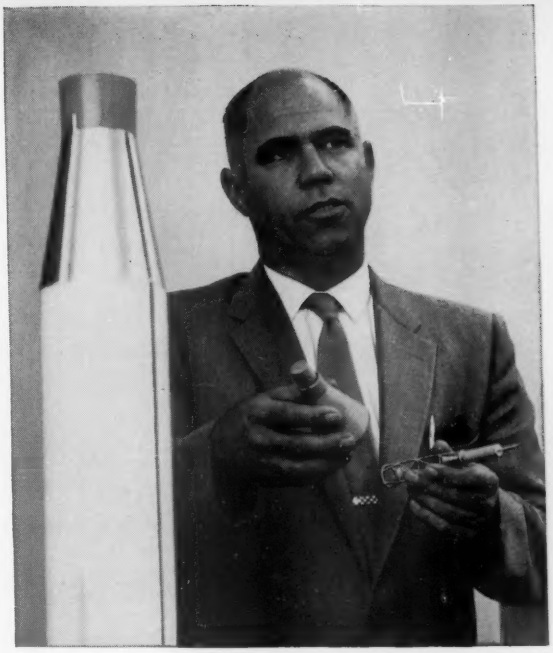
How New Landing Systems from Radioplane Meet the Demands of High Altitudes, Increased Bail-out Speeds, and Recovery from Space



by Ed Ewing

Engineering Specialist, Assigned to Project Mercury at Radioplane Division, Northrop Corporation

In high-speed bail outs, the opening process of the standard personnel parachute is a nylon explosion. The fate of the man with his body harness attached to the risers depends mainly on the magnitude of this opening shock and his body position when he receives it. Today's increased speeds and altitudes (where parachutes open even faster) have made opening shock a serious hazard to survival.

An emergency ejection from a high-speed jet, for example, is a sudden thrust into the full blast of the airstream at speeds up to 800 miles per hour – enough to rip open most parachute containers and tear their contents to shreds. And because of the airman's disorientation during the shock of ejection, the most reliable system must place minimum dependence on human intelligence for its operation.

When the U.S. Naval Bureau of Aeronautics outlined its needs and set its requirements for an improved personnel parachute, industry-wide attempts were made to meet the challenge. Radioplane won the contract.

Radioplane took a new approach to the design and development of a canopy that would open just slowly enough to bring the shock within tolerable limits and at the same time open positively and dependably. Radioplane experimented with five different models, seven modifications and 270 dummy drop tests to produce the now-famous "Skysail"—the ring parachute with the unique sawtooth profile.

Proved in more than 300 qualification jumps, "Skysail" opens one ring at a time starting from a small bubble in the crown. The leading edge of each ring bites into the air in a succession of deliberate step-by-step openings that takes an important fraction of a second longer than the explosion-like filling of the standard canopy. The resultant reduction in opening shock is between 35 and 50 per cent, the drag coefficient is 20 per cent higher than that attained by other parachutes of equivalent opening force and stability.

"Skysail" proves to be the solution to a challenging phase of the jet age.

For the space age, Radioplane is already delivering "Ringsail" – the landing system for America's first man-in-space capsule – NASA's Project Mercury.

As new needs and new challenges arise, the Radioplane scientist, specialist or engineer is in a position to develop and use his creative talents freely. Besides working on escape and landing systems, he engages in scope-widening studies in re-entry mechanics, hyper-environments and physics of materials. Radioplane fosters an atmosphere in which he is urged to develop new ideas and new techniques in the missile, pilotless aircraft, and space recovery fields. With Radioplane's outstanding facilities, colleagues, and current programs to encourage him, horizons for the individual are wide at Radioplane. They are wide to allow outstanding ingenuity and creativity full range to advance.

Current papers by Northrop scientists and engineers include:

"Disintegration Barriers to Extremely High-Speed Space Travel" by Dr. Elliot T. Benedikt.

"An Astrovehicle Rendezvous-Guidance Concept" by Norman V. Petersen, Robert Swanson and Leroy Hoover.

For copies of these papers and additional information about Northrop Corporation, write:

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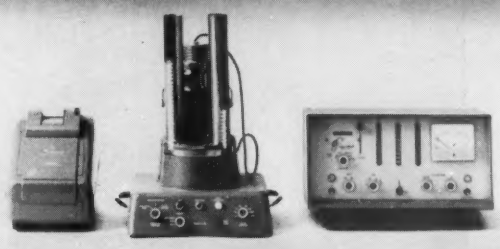

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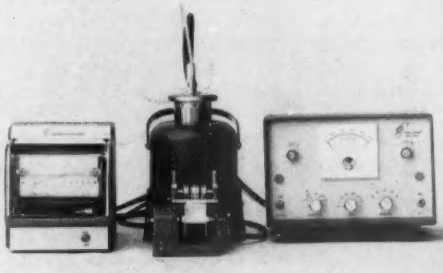

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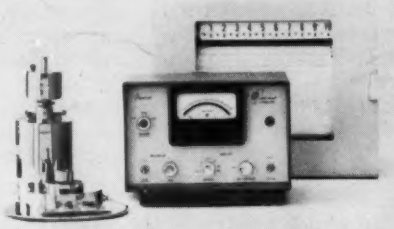

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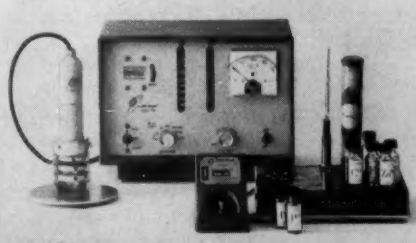

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